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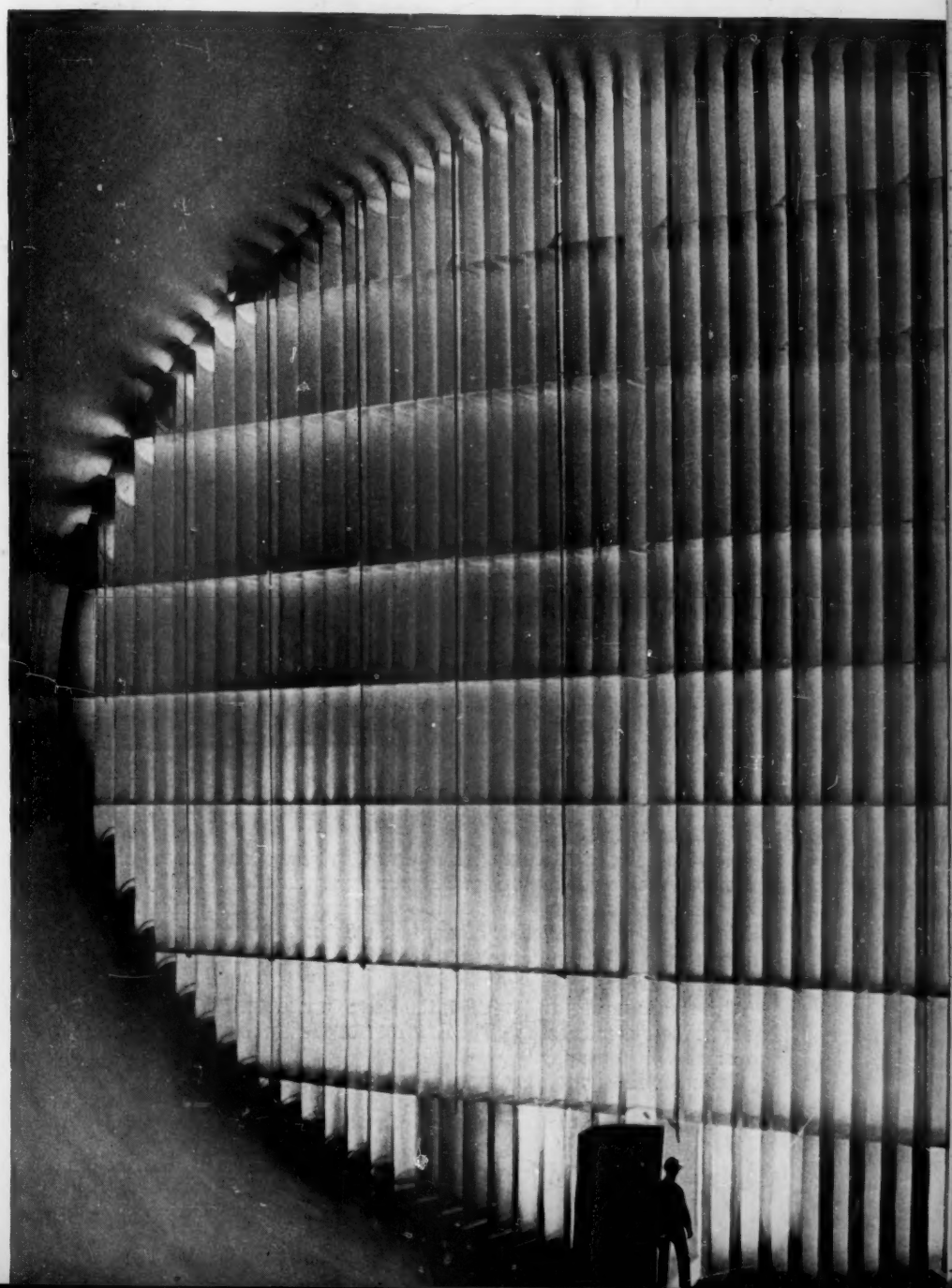

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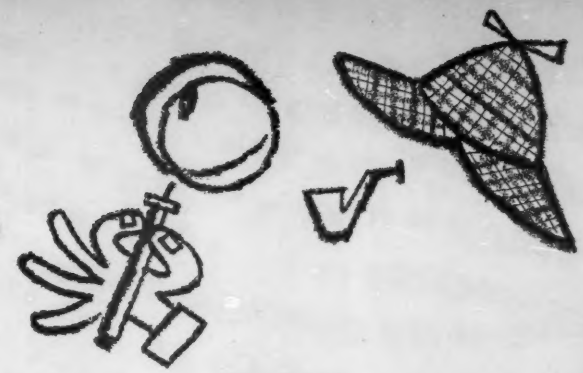

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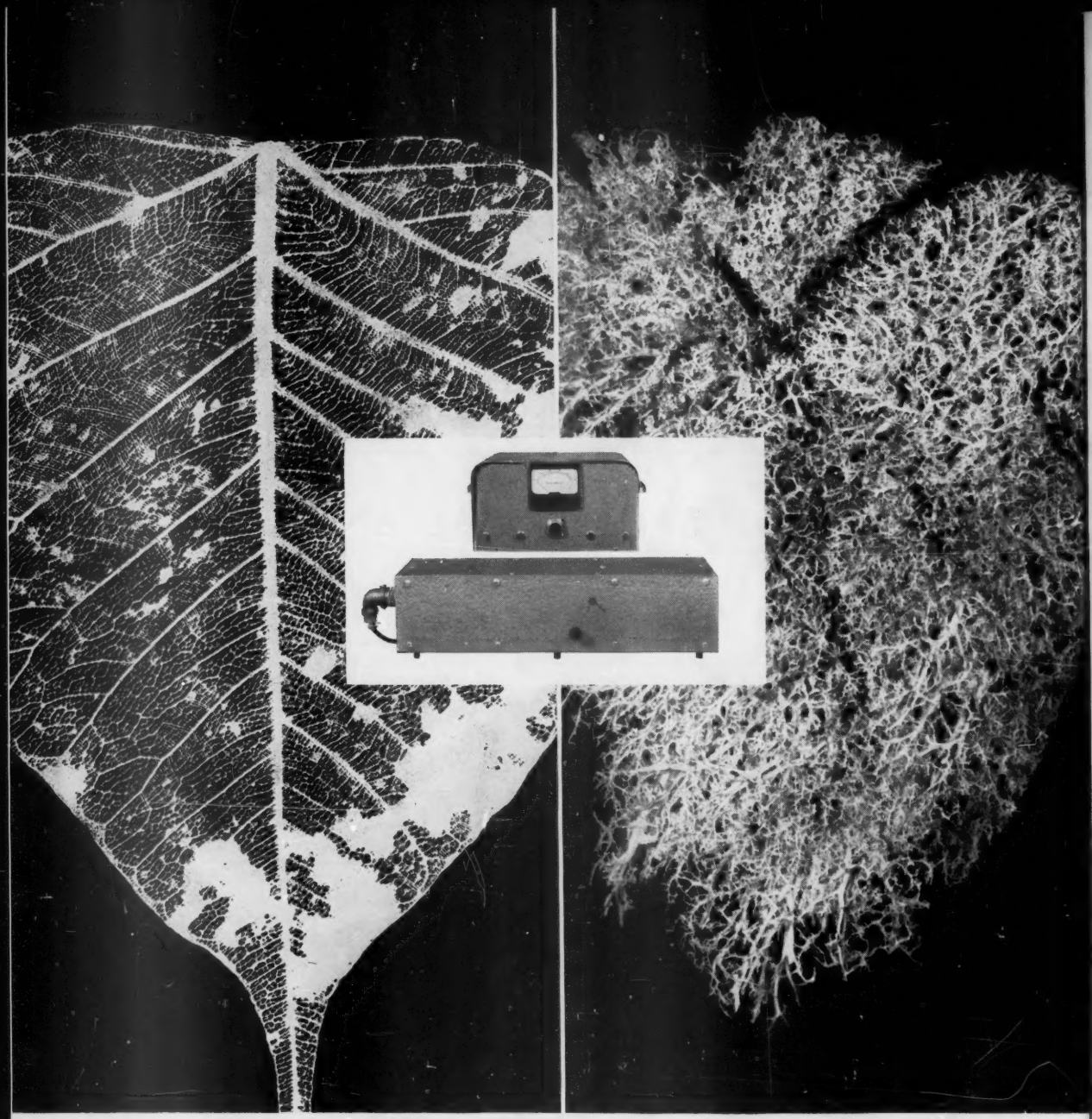

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Cover

Giant turning vanes resembling oversized Venetian blinds are set in each corner of the transonic circuit of the propulsion wind tunnel at the Arnold Engineering Development Center at Tullahoma, Tenn. The vanes guide air flow around the 90° corners of the circuit to reduce turbulence and help maintain a steady flow of air through the test section. The circuit operates in Mach number range from about 0.5 to 1.6 at simulated altitudes up to and beyond 100,000 feet. [Arnold Engineering Development Center, U.S. Air Research and Development Command]

California

Letters

Imprinting

While I wish to follow Eckhard Hess' injunction [*Science* **130**, 133 (1959)] of making no assumption about the nature of imprinting, may I suggest the concept of "deflection" for the time when such speculations will become helpful. The instinctual need for parent relation can be channeled into a variety of objects. I have previously designated such modifications as deflection [G. Haydu, *Architecture of Sanity* (Julian Press, New York, 1958)]. Imprinting is a particularly fascinating form of deflection.

Hess' studies with meprobamate and chlorpromazine (see his Table 1) are in good accord with this concept. Meprobamate reduces only the immediacy or urgency of the object need but not its form. Chlorpromazine brings the organism back from its present configuration and lets it transact such object need in an earlier style. (In the rat, for example, chlorpromazine inhibits the deflected form of escape drive.)

In his Table 1, line 2, Hess indicates that meprobamate reduces imprinting attempted at an optimal period, while chlorpromazine does not do this. The regressive action of the latter brings the duckling back to a period during which receptivity is high. Line 4 of the same table indicates that meprobamate produces no effect when object need for deflection is at a normally low level, while chlorpromazine effects a marked increase of imprinting by bringing the duckling back to a previous period (17 to 20 hours) which is characterized by considerable "imprintability."

GEORGE G. HAYDU
Creedmoor Institute for Psychobiologic Studies, Queens Village, Jamaica, New York

In the article entitled "Imprinting" [*Science* **130**, 133 (1959)] by E. H. Hess, experiments contrary to the author's theoretical position were dismissed because of the alleged failure to employ animals of an appropriate age. Thus Hess states: "In many instances the animals are *too old* when used in the experiments to fall within the critical age for imprinting, with the result that only association learning can occur" (italics mine).

In one of the studies which Hess dismisses [H. Moltz and L. Rosenblum, *J. Comp.* and *Physiol. Psychol.* (1958)] it is stated quite explicitly that all ducklings were exposed to the "imprinting" object within 5 to 10 hours after hatching. Since the critical period to which Hess refers occurs during the first 13 to 16 hours from hatching, Moltz and Rosenblum's animals were obviously not too old. Therefore, Hess' dismissal of the Moltz-Rosenblum data—data which are not in accord with his theoretical position with respect to imprinting—was not justified. A similar error was made in dismissing two studies by Jaynes [*J. Comp. and Physiol. Psychol.* (1956); *ibid.* (1957)]. Again, the subjects employed were not "too old," and the data were contradictory.

An article of mine, currently in press in the *Psychological Bulletin*, treats the experimental results which Hess does not consider relevant to his position and presents a different explanation of the imprinting phenomenon.

HOWARD MOLTZ
Department of Psychology, Brooklyn College, Brooklyn, New York

The Newtonian Universe

In a recent issue of *Science* [**130**, 328 (7 Aug. 1959)], M. K. Munitz reviews Arthur Koestler's book *The Sleepwalkers*. As I have not read the book, I cannot either agree or disagree with Koestler. One of Munitz's notions, however, calls for comment. He quotes Koestler as saying that "the blueprint of the universe remains essentially the one Newton drew for us, in spite of all disturbing rumours about the curvature of space, the relativity of time, and the runaway nebulae." Munitz adds to this that "the judgment just quoted is . . . contrary to the most obvious truth and in its own way . . . perverse . . .," and he indicates that in his view modern science has gone far beyond Newton to things radically new.

It seems to me that there is here a strong misunderstanding as between the constructs of the professional scientist and the somewhat simpler conceptual universe of the intelligent and educated layman. It is no accident that, only two generations after Newton, Kant and Laplace proposed a vast universe filled with innumerable stars at large distances from each other which have condensed to their present form out of diffuse gaseous masses, a concept which we still hold today in some only mildly modified form. This line of thought is entirely "Newtonian." The main thing that we moderns have added is the burning up of nuclear fuel to keep the stars shining. It seems clear that the revolutionary break is between the universe of Aristotle and Ptolemy on the one hand and that of Kepler and Newton on the other, relativity being also very near to Newtonian concepts. So far as I can see, there is here a danger that we professionals lose touch with the philosophy and the concepts of society at large. The loss will be both society's and ours.

WALTER M. ELSASSER
University of California, La Jolla

Stored Foodstuffs and Fallout

Because of the high concentration of radioactive fallout in bombed areas after a nuclear attack, soils would be contaminated and crops could not be grown for a considerable period. The effects of this critical period of possible starvation could be mitigated by utilization of so-called surplus grains, milk, and egg products from storage. The shelters that house the foodstuffs would keep them from being contaminated by strontium-90 and other long-lived radioisotopes. These storage structures could be decontaminated far more easily and more quickly than the soils on which crops would have to be grown.

Much attention is being given to the problem of the accumulation of large supplies of surplus grains, milk, and egg products in storage in this country, and to the cost of maintaining the proper storage conditions to insure against spoilage. Granted that these costs are high, such surpluses nevertheless constitute the cheapest kind of insurance against possible starvation periods that would follow an atomic holocaust. In fact, protective storage of foodstuffs seems to be the only means of alleviating, at least to some extent, critical food shortages in contaminated regions.

The normal production of new foodstuffs would probably be suddenly and completely cut off in extensive areas beneath bomb bursts. Data are at hand that show how long the present stored surpluses of foodstuffs would supply the population. These data show that stored supplies would be consumed in a comparatively short time, even when rationed at a bare maintenance level.

For these reasons I advocate that the present levels of stored foodstuffs be carefully maintained. More than that, a study should be made of possible means of improving storage against fallout contamination. In addition, consideration should be given to the advisability of increasing the stored amounts of some foodstuffs and possibly decreasing others. Maintaining reserves of fiber materials should also be considered, particularly of materials needed for clothing.

J. R. NELLER
University of Florida Agricultural Experiment Station, Gainesville

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Tax Exempt

The first Federal income tax law of 1913 recognized that institutions promoting scientific research were entitled to tax exemption. Since at that time scientific research was largely restricted to universities, which were also exempt on educational grounds, no special problems about the meaning of the word "scientific" for tax purposes arose and the word was, in fact, not defined.

The extraordinary growth in both applied and basic research in the intervening years has not been confined to the universities: numerous non-profit institutions, some of which are independent of university affiliation or control, have been founded to conduct contract research for government and industry. Their existence has led the Bureau of Internal Revenue to attempt a definition of what "scientific" means for income tax purposes.

A proposed amendment to the Income Tax Regulations (*Federal Register*, 24:9587, 1 Dec. 1959), which will probably be discussed at hearings in the Internal Revenue Service this month, sets forth the main criteria to be used in determining whether an organization shall enjoy tax exemption on scientific grounds. The major criteria are that the organization be operated primarily for fundamental research and that the results of that research be freely available to the public. Thus, an oganization will not be eligible for tax exemption if its primary aim is to do routine testing or applied research (unless such research is done for national, state, or local governments) or if it serves a private rather than a public interest. The overriding consideration is whether the research "is directed not toward promoting private gain, but rather toward benefiting the public."

The provision for free availability to the public means that the research results must not be subject to any restriction or proprietary rights and "are made available to the interested public in a practical manner and on a non-discriminatory basis." Any research done for a governmental agency will be considered freely available even though it may in fact be restricted for security or other reasons.

What organizations will be affected? Universities, hospitals, and organizations operated *primarily* for carrying on fundamental research that is freely available to the public will not be affected. If an institution meets this test it is exempt and the exemption includes income derived from applied research that is not available to the public.

The proposed ruling will thus affect those independent research institutions that are predominantly concerned with applied research, the results of which are restricted or controlled by those who pay for it. Many such institutions now claim exemption and they may be expected to marshal a strong attack against the proposed regulation. They will doubtless point out that their performance of research by contract for government and industry is in the public interest and that it is "scientific" for purposes of tax exemption. If the regulation nevertheless goes into effect they may appeal through the courts. If this fails, two courses will be open to them: they may either pay tax on that part of their income from research that is not exempt or perform enough basic research to maintain an exempt status.—G.DuS.

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Development of a Science Policy

Steps are needed to center full responsibility for government programs in the government itself.

Wallace R. Brode

"The liberal spirit which animates both Congress and the executive departments in their dealings with scientific affairs is very apt to lead them into the support of scientific enterprises without any sufficient consideration of the conditions of success and of efficient and economical administration; and a careful consideration of each proposed undertaking by a committee of experts is what is wanted to insure the adoption of the best methods."

These words appeared in an editorial in *Science* published 25 April 1884. The magazine *Science* was then only three years old, the American Association for the Advancement of Science was 36 years old, and there were only four or five government agencies active in science. What prompted this editorial of 75 years ago were the lively discussions on the need for a Department of Science in the government. Then, as now, the scientists as well as other government officials were divided in opinion. In 1884 Congress appointed a commission, known as the Allison Commission, to consider the creation of a Department of Science. The President of the National Academy of Sciences, O. C. Marsh (a former AAAS president), was asked to name members of the academy to serve on a committee to assist the Allison Commission. Among the members named were two distinguished scientists, Simon Newcomb and Cyrus Comstock. This committee was to survey and study the procedure of handling science in other countries and to recommend methods of coordinating the scientific areas. Simon Newcomb had served as president of the American Association for the Advancement of Science seven years earlier, in 1877, and was one of the Navy's most illustrious scientists. General Cyrus Comstock was an equally distinguished academician who served in the Army. However, the Secretaries of the Army and Navy both objected to a government scientist from their agencies serving on a National Academy of Sciences committee which was to advise the government.

One of the most outspoken in favor of a Department of Science was Major John Wesley Powell, a vigorous and colorful government scientist, who was chief of the Geological Survey, a nonmilitary establishment. Powell appeared on 16 occasions before the congressional committee. He commented on the elimination of Newcomb and Comstock from the academy committee and noted that the "military officer plans and commands; the civil officer hears, weighs and decides," and that "the military secretaries did not desire to have their subordinates deliberate on questions of policy affecting the conduct of the secretaries themselves." Powell, however, felt this suppression was justified in the military circles but would not have been justified in a civilian area.

The National Academy of Sciences committee expressed the feeling "that the time is near when the country will demand the institution of a branch of the executive government devoted especially to the direction and control of all of the purely scientific work of the government." However, if establishment of a department could not be effected, they felt that a coordinating scientific commission would be in order. Neither a Department of Science nor a Science Commission was established, due to various factors, including political changes in a new Congress.

Powell supported a Department of Science, although he favored some modification of the proposals of the Allison Commission. Powell was elected president of the American Association for the Advancement of Science in 1888, and I had thought that his presidential address before the association in 1889 might add some comment, in retrospect, to his testimony of four years earlier on a Department of Science. However, Powell's presidential address, published in the American Association for the Advancement of Science's proceedings, was a scholarly ethnological discussion on "Evolution of music from dance to symphony"—a subject on which he, as founder of the Bureau of American Ethnology and a student of American Indian culture, was most competent to address this association.

The very fact that the organizational issues being considered 75 years ago are basically identical to those of today raises this fundamental question: Has science changed in relative importance over the period of years since the founding of our country? Jefferson, as Secretary of State in our first cabinet, was in a sense also Secretary of Science because he handled such areas as patents, decimal coinage, and our standards of length and weight. Each major scientific or technological development has in its period of history created startling and revolutionary changes in the daily pattern of life. The telegraph as compared to the pony dispatch probably represented a more radical advance than did the telephone over the telegraph, the

Dr. Brode, retiring president of the AAAS, is science adviser to the Secretary of State, Washington, D.C. This article is his AAAS presidential address, which was given on 28 December 1959 during the 126th annual meeting in Chicago, Ill.

radio over the telephone, or television over the radio. The electric light had just been introduced in 1884, and there appeared in the literature pronouncements about the "impact of science" which are as current as if made today.

Although it would appear that problems which existed earlier are still with us today, they are actually compounded. Whereas Thomas Henry, Asa Grey, Wolcott Gibbs, and others in the past produced their discoveries and theories as individuals, many of today's advances represent the work of teams, whole laboratories, and industries. This complexity in the scope and size of scientific operation will not remain static but will continue to increase dynamically.

Hence, the concern of the scientific community is justified. If one feels that present arrangements are unwieldy enough today, what will their state be in another 100 years?

Recent Developments in a National Science Program

The American Association for the Advancement of Science has led in exploring these problems through its Parliament of Science, its Basic Research Symposium, its regular meetings, its programs, and its publication *Science*. These have all helped to stimulate the consideration of our science policy. In the past two decades there have been a number of governmentally appointed boards, panels, and commissions, to evaluate the place of science in our nation and government. Nearly 15 years ago Vannevar Bush, as the coordinator of the nation's defense research, issued his famous treatise *Science, the Endless Frontier*. In 1946 the President of the United States created a President's Scientific Research Board, under the chairmanship of John R. Steelman, to study science and public policy; and in 1947 this board issued a four-volume work known as the Steelman Report, under the title *Science and Public Policy*. Six years ago the Commission on Government Reorganization, also known as the Hoover Commission, was concerned with our expanding government. In 1956, the President's Committee on Scientists and Engineers was created; in 1957 the President's Science Advisory Committee was established, and upon the recommendation of this body in 1959 the Federal Council for Science and Technology came into existence.

All of these groups have devoted considerable time and energy to various phases of government operation and the role of science. The National Science Foundation is a direct result of recommendations made by Bush and the Steelman Report. As a result of the Hoover Commission studies there evolved the Department of Health, Education, and Welfare, which encompasses major social and health agencies, but whose ultimate formation was not accomplished without considerable controversy and discussion.

Priorities—Imbalance

A recent editorial in one of our nation's leading papers notes that chairman McCone of the Atomic Energy Commission "is convinced that the United States—if it is not to become technologically and economically inferior to the U.S.S.R.—must work out methods of marshalling its scientific and technical talents for concentrated *top priority* work on projects of overriding significance." McCone further proposed that this increased activity in a specific scientific field should be made "at the expense of projects of lesser importance." If one did not know the scientific subject matter of chairman McCone's agency, one might well ask, "Is this space, oceanography, undersea geology, meteorology, medicine, education, high-energy physics, atomic energy, food, materials, weather and smog control, or transportation?" Each of these areas has supporters who feel that programs varying from ten million to ten billion dollars a year are essential to scientific progress.

This *overriding* or *top-priority* attitude of some specialists is a reflection of the enthusiasm for one's own field of specialization. However, a crisis has been reached in this overlapping expansion and growth of our science programs, so that none of these major programs can be adequately supported except at the expense of the less glamorous areas of science, education, and culture, which are, nevertheless, essential to our basic welfare.

Why have we arrived at this state or crisis? The answer is *expediency*. The immediate conditions and circumstances existing at the time determine which programs are "top priority." Even when the stimulating conditions have been removed, the top-priority label is often maintained. Then another set of circumstances dictates the creation of another top-priority label assignment to another area of science.

Even though "atoms for peace" is slowly being transformed into "science for peace," there do exist separate areas of nuclear and atomic research which enjoy an elevation high above science in general. Many less well developed nations have devoted time and resources to nuclear and atomic programs rather than basic science. There are areas of the world where basic education, health, and agricultural training should predominate, but we find these countries building nuclear reactors by crude methods of hand labor. When we ask what is to be done with the reactors we are advised that they are training reactors, to train people to run more reactors.

The latest area of science which is capturing the minds and purse of our nation is space. There are a number of good contenders in the race for future top-priority assignments. Perhaps the next will be oceanography, weather, or materials research. We do favor advances in these areas of science; we need this progress, but this progress should not be effected through a corresponding reduction in the rate of advance for other equally important areas of science.

How can projects which are "overriding" and those which are of "lesser importance" be identified? Who is to make this allocation of relative effort? One of the most difficult tasks facing us is to achieve a long-range planning effort which would remove expediency as the sole controlling factor. A national science policy is needed for a wise and rational distribution of scientific activities, so that space, defense, education, atomic energy, oceanography, and medical research are not bidding against each other for limited available support. The growing demand for scientists in the face of a limited supply of scientists, materials, funds, and facilities requires major policy decisions as to the distribution of resources. These decisions should of course include the extent to which specialized agencies may recruit by scholarship, fellowship, and research support.

Every enthusiastic scientist with a dream for the future can envisage space ships at his command; areas of flashing lights and computing machines reading, translating, abstracting, and digesting the world's literature, even solving the problems punched into the machine; or reflecting radio telescopes a mile in diameter to enable him to communicate with other worlds. However, there must

be a limit, and not only must scientists realize that there should be a relative priority assigned to areas of science but there should also be recognition that scientific programs do not all have priorities that override economic, political, educational, and social developments.

With tongue in cheek, a past president of the American Association for the Advancement of Science, Warren Weaver, approached this problem of super-programs and priority assignments in a recent article in *Science* [**130**, 1390 (20 Nov. 1959)], in a clever satire on the "Report of the Special Committee." The "Weaver Report," along with Parkinson's Law, may provide the means for arriving at some solution to our problem, by taking a distorted view and working backwards toward a rational solution.

Current Research Support

We have seen a marked shift in the responsibility for the support of scientific research in the past 30 years. In the 1930's the government was supporting only about 15 percent of the nation's basic research, which was almost exclusively in its own laboratories. Today, the federal government is supporting about 85 percent of the nation's basic research, of which still only about 15 percent is in its own laboratories. This great growth, in both percentage and total amount, has been primarily in research support in industrial and educational contracts. Today our government's total budget is many times larger than the budget of the 1930's, and four-fifths of this budget is for defense activities and less than one-tenth, for normal governmental activities. The expenditure of 1 percent of the defense agencies' budget (and I am including the area of applications of atomic energy and space research as defense) in support of research means about $500 million a year, which is about 85 percent of the nation's program in research. This means essentially that our scientific research program is directed and guided by these agencies.

It is certainly true that after World War II there was an emergency situation and that without the aid given our educational and research programs by government agencies concerned with defense and applied sciences we would be in a sorry mess, unless—and this is the great unknown—the pressure of the situation would have been sufficient to create a more logical civilian support to this civilian activity. It is generally agreed that the initial "bailing out" of universities by the opulent agencies required only a small portion of their total budgets and was a good thing in the immediate situation. Just how much this expedient delayed general support to science education, coordinated science programming, or a national science policy is unknown and may never be clearly recognized.

The Steelman Report recommended that the National Science Foundation should support basic research in governmental establishments as well as in universities, yet the pattern of today's grants is essentially to universities and does not combine what could be the government's broad interest and coordination of the entire national science program.

In many of the basic-science areas of the government, such as the National Bureau of Standards, the Weather Bureau, the Geological Survey, the Bureau of Mines and the Forest Products Laboratory, which are attached as appendages to major departments, personnel supported by direct appropriation are essentially at the prewar level of the 1930's. The growth of the bureaus needed to keep up with our expanded science program is almost entirely dependent, as have been our universities for support in research, on contracts from the large agencies concerned with applied sciences and defense. Another justification for the continuance of research support both in universities and in nondefense government laboratories by the major agencies concerned with defense and applied sciences has been that with multiple supporting agencies, contractors can shop for different sponsors. Since this research is only a minor function of the supporting agency and often is not directly related to its mission, less coordination might be expected than if all funds came from a single agency whose principal function was to support our national science and educational program.

In the establishment of the National Science Foundation many of us who followed the discussion of its formation felt that the nation's responsibilities in basic research would, to some extent, be absorbed from other government agencies concerned with applications who had assumed some of these responsibilities. To prevent a drastic movement of the support of basic research to the National Science Foundation, it was actually indicated that the various agencies concerned with applications should continue to support, in their own organizations, "basic research in areas which are closely related to their mission."

There was no intention of removing basic research from agencies and laboratories which require it in their developmental activity. In fact the thinking scientist has often been worried to find that there did not appear to be a sufficient guarantee of adequate interest and support of necessary basic research in the agency establishments concerned with applications.

Most of our technological agencies have special authorizations in their enabling acts permitting them to engage in basic research in support of the objectives of their mission. So far as I know there has been no question raised as to whether such research was proper. However, many technological agencies have used such a research authorization to justify their support of research programs in all areas of science rather than in those areas which would appear to be directly concerned with their proper mission. Even the National Science Foundation, which has a rather broad mission concept, is aware that certain phases of science, such as medicine and agriculture, are in the areas of responsibility of other government agencies.

Nearly all of the agencies concerned with applications have indicated that they feel a responsibility to help train the research workers of tomorrow through scholarship and research grants. If such agencies strongly felt a responsibility, they would show no reticence in providing such funds to the universities as university grants, or in transferring these funds to the National Science Foundation to augment a planned national program of general support. What is disturbing is an insistence by each agency that its granting office should be able to select and direct both the recipient and the subject of research so as to exercise a guiding hand in our educational and research institutions. These amounts are not small, for they constitute much more than half of the total support to basic research in this country.

The problem which concerns us is that Army, Navy, Air Force, and space and atomic energy agencies have had to assume responsibility for contracting for this very extensive amount of basic research in universities, foundations, and government laboratories both here and abroad in all phases of science. While most of these supported groups and many scientists in this country do not

question the source of the "money in the collection plate," it has become more apparent to me in dealing with foreign science programs that there is hesitancy abroad on the part of scientists or universities at becoming involved in programs supported by a foreign military agency. This is especially so where the country is essentially neutral or where the program is not a part of a mutual defense act in which the scientist's own country is participating and to which the proposed program is attached.

With the great increase in recent years in the support of basic research by public agencies and foundations and the predicted doubling of research in the next ten years, there is reason to feel that a more orderly way of funding might be evolved than reliance on a multiplicity of sponsors. The urgency of the need for a revision of our program methods becomes greater as the size of programs increases so as to involve large sums of money, large numbers of individuals, and many research facilities. The pressures for priority action on massive programs, such as those concerned with space ships, new sources of energy, or weather control, often develop in the nonscientific political or economic areas and work their way towards the university, industrial, and government research worker.

Obligation To Continue Support

Many of us are concerned, as was Eliza Doolittle in Shaw's *Pygmalion* (and in *My Fair Lady*), who plaintively asked "What's to become of me?" when, after being raised to a level of competence and ability, she was threatened with the possibility of being cut off from her subsidized support and faced the prospect of being heaved out on the streets and having to shift for herself again.

The government has a responsibility, which will certainly grow in size and scope, to support a major share of the nation's research and applied-science programs. This responsibility in the area of advanced scientific education and basic research in universities, institutes, and government laboratories must be fully met if we are to maintain a technological leadership in the world.

There should be a revision and realignment of our support so as to provide more direct and less controlling support to universities, and greater direct support to government basic-science programs in the government's own laboratories. Serious consideration should be given to the reduction or elimination of "convenience" or synthetic scientific agencies which, while operating as nongovernment laboratories or institutions, are doing almost exclusively governmental work with government funds. These laboratories should be made bona fide government laboratories, and they should be so directed and set up as to permit the government to do its own essential scientific work under working conditions which are most conducive to efficient and effective operation.

I feel that the government does have a responsibility to continue to support science, just as it has a responsibility for health, agriculture, and defense. There must, however, be some instrumentality with a considerable degree of control, which can decide when to support, when to taper off, or when to terminate various research programs—and such responsibility must eventually center in a coordinating establishment such as a Department of Science.

Government Scientists

If we are to maintain in the government high-level policy and research positions for scientists we must provide compatible employment and a challenge to their capabilities in the responsibilities assigned them. It is difficult to attract or hold good scientists if there is no future level of administrative or research responsibility which they can expect to reach. Much of the government funds for applied research for the government is used to maintain non-civil-service laboratories or organizations such as Los Alamos, ARPIA-IDA, Lincoln, Brookhaven, Oak Ridge, the Applied Physics Laboratory, and many industrial laboratories. These quasigovernmental laboratories provide higher salaries and better working conditions than are provided scientists in the government and—perhaps most important to the scientist—provide high-level, polity-type positions of responsibility, and the employees are treated as nongovernmental scientists in dealing with the government. It would seem that a number of these contract operations in science and technology should be reexamined to determine whether there should be an improvement in the status of the government-employed scientist and direct absorption of much of this work in a Department of Science or other coordinating science structure in our government.

One interesting recommendation of the Steelman Report was that the board of the National Science Foundation should be composed of "distinguished scientists and educators to be drawn one-half from the Government and one-half from the outside." Actually the board includes no government scientists but is made up exclusively of university and industrial leaders, with university personnel predominating. In selecting personnel to run government departments, bureaus, or offices it is generally the procedure to try to find someone to bring in from the outside. Usually such a person is on leave of absence from his industry or university for one or two years. Seldom is the appointment of a career civil servant seriously considered, even though there are in the government career civil servants who may be recognized in other nations as world authorities and leaders in the field.

There should be established a policy whereby top management is not maintained on a rotational system by persons who are not directly associated with government operations. It is certainly true that many a highly respected and competent university professor, lawyer, or businessman has at considerable sacrifice of time and money, and often of prestige, agreed to come to Washington for a one- or two-year period to pitch in and help run the government. Nevertheless, the government should develop within its own establishment sufficient capability to operate its agencies, and our policy decisions, whether they be in science, taxation, or welfare, should not be made without guidance from personnel experienced in governmental operations.

Determination of Future Policy in Science

The problems to be faced are these: (i) determination of the direction in which science will advance and of the areas in which continuing or new programs are to be supported; (ii) the emphasis and relative priorities to be placed on scientific programs, including not only the "top-priority" programs but also the minor programs which need to be kept alive and operating on a modest scale; (iii) the administration, financing, evaluation, and support of our science programs within the government; and (iv) the distribution of responsibility for the carrying out of scientific programs between government laboratories and university, private, industrial, domestic,

and foreign (intergovernment, government, and private) facilities.

The directions in which science should be encouraged to advance will in part be spontaneously determined for us by the inquisitive research worker who has been given freedom to probe. Essentially this is exploratory research and should be supported by institutions, so that research workers may spend a reasonable portion of their time in exploration. In addition to this limited area of free research there is our major area of programed research, for which there should be some over-all plan. Such a plan must establish the relative priorities or emphasis and the rates at which certain programs should be pursued.

Our problem today is that we have reached a saturation point with respect to available personnel. Hence, further expansion or support in many fields must of necessity require reduction of the active available material in other science programs. Such a disturbance in our present unstable equilibrium of distribution of effort is particularly felt when massive new programs are initiated, such as new billion-dollar efforts in space, oceanography, or health.

The Hoover Commission noted that our government is expanding to such an extent that the executive branch has 74 agencies "which divide responsibility and which are too great in number for effective direction from the top." The commission recommended that certain of these should be "grouped by related function under the heads of departments." They indicated "in many cases several agencies each have a small share in carrying out a single major policy, which ought to be the responsibility of one department." In summary they concluded that the government must "create a more orderly grouping of the functions of government into major departments and agencies under the President."

The Steelman Report in discussing the creation and operation of a proposed National Science Foundation suggested that the Foundation "should be located within the Executive Office of the President until such time as other federal programs in support of higher education are established. At such time, consideration should be given to grouping all such activities, including the National Science Foundation, in a single agency." The National Science Foundation, which was subsequently established, has a more scientific than educational mission, and with the establishment of other new science agencies, such as the space agency, it would appear that the Steelman recommendation might well be applied in principle toward the creation of a Department of Science.

Coordination of Science in the Government

There is little if any opposition to the broad concept that as scientific or any other activity grows in size and complexity and begins to be a major consumer of personnel, funds, and facilities in our economy and culture, some consideration should be given to increasing efficiency in the utilization of our limited resources through suitable coordination and planning.

The President's Science Advisory Committee, a group of nongovernmental scientists, in viewing from outside the government the problem of science in government, suggested that there should be created some instrumentality to promote closer cooperation among federal agencies in planning and managing their program in science and technology. They recommended the establishment of a Federal Council for Science and Technology, and the President issued an executive order accordingly, in March 1959. This council is an inside-the-government group consisting of policy members of departments or agencies concerned with science. The representatives, however, need not be scientists, and as the council is presently constituted, many are not scientists.

It would seem reasonable that the Congress should be able to seek advice and counsel from a coordinated science leadership in the government. Early this year the chairman of the congressional committee held hearings on a possible Department of Science and sought the appearance as a witness, for advice and comment, of the chairman of the newly created Federal Council for Science and Technology. The council chairman declined to appear, on the grounds that he was a privileged member of the President's staff, yet as head of the Federal Council for Science and Technology he was responsible for the organization which was charged with making recommendations for the creation of effective means of promoting a more efficient, coordinated science program in the government. It was my own opinion, which I expressed when, as president of the American Association for the Advancement of Science, I was asked to appear before the same congressional committee, that a strong and responsive Federal Council for Science and Technology might well evolve into a Department of Science.

The inability of the Congress to draw on the advice of the existing Federal Council for Science and Technology is in itself an indication of the need to separate our science coordination direction from the President's own office and Science Adviser so that the coordinated leadership in science in the government may speak with the authority of the group it represents rather than only through the President. I feel that the President should continue to have a strong science adviser, however, in the presentation of a science program for the government. The leader of the program should be able, like the heads of other agencies, committees, departments, or commissions, to speak for his agencies before the Congress, and also to report the findings and recommendations of those agencies to the President. It would seem that in the organization of science and our governmental science policy, as in the areas of labor, commerce, the military, health, education, and agriculture, it should be possible for the planning and organizing committees of Congress to have reasonable access to the agency in question for advice and assistance.

The President's Science Advisory Committee considered the suggestion of a Department of Science as a proposed solution to the problem of coordinating the nation's science programs but defined a Department of Science as a department which would bring together *"all* of the government's numerous scientific and technological operations," including the scientific phases of defense, agriculture, and health which were directly related to the missions of the agencies with responsibility in these areas, and pointed out that such operations "could not be satisfactorily administered by a department far removed from the problems that are to be solved." This is a synthetic defense in that elimination of research or development work pertinent and essential to the proper missions of these agencies was not proposed or recommended by the promoters of the legislation actually before Congress. The proposed legislation for a Department of Science was somewhat nebulous, but its main purpose was to stimulate discussion, and its originators recognized that it did not present a Department of Science concept that would be fully acceptable to all.

I consider as not of significance the

arguments presented in the congressional hearings in which those who opposed a coordinated science program maintained that such a program would place too much centralization of authority in a single agency and establish too much government control. Neither argument is tenable if proper administration is provided and safeguarded.

A corrupt and mismanaged program would be bad, but to argue against any coordination on the grounds that it *could* be bad is just not logical. We should favor a governmentally supported program and at the same time incorporate into such a coordinated program the essential safeguards used in our local, state, national, and international institutions, both governmental and private.

Belgium, France, South Africa, and England have created cabinet or semicabinet posts for science departments. England has already indicated the nature of agencies to be absorbed into such a collation. These include space, atomic energy, health, research grants, and specialized science agencies such as standards, weather, patents, and science information. In his first press conference Lord Hailsham, Lord Privy Seal and the new Minister for Science in the British Cabinet, pointed out that "whether or not there is a need for a Minister or Ministry [in science] . . . there is a need for a policy in science and that policy cannot be a product of government thinking alone." Lord Hailsham emphasized that his Advisory Council on Science Policy "provides one of the keys to the present situation composed as it is of a unique connexion of Government and non-Government scientists. . . ."

Pioneering in the creation of a Department of Science in a government took place in this country about 100 years ago, under somewhat unusual circumstances. In the process of reviewing material for this presentation I became interested in how many presidents of the American Association for the Advancement of Science had been government scientists for a considerable portion of their careers. I found that about 30 out of the 112 had been government scientists. One of the former presidents of the American Association for the Advancement of Science was John L. LeConte, in 1874. In looking up his background I was intrigued to find that he had two cousins, Joseph LeConte and John LeConte, who had served as members of the Science Department of the Jefferson Davis government during the Civil War. Joseph LeConte also became president of the American Association for the Advancement of Science in 1892.

If we accept the concept that we need a national science policy to guide our scientific effort, and if, as a result of suitable commission and congressional action, formation of a Department of Science is proposed, we should be certain that it is in fact as well as in name an operating department. It should not be a superstructure imposed on existing organizations, but it should represent an honest and real effort to mesh the scientific interests and objectives of our government in the fullest possible utilization of resources. Thus, a Department of Science, while not removing from agencies such as Defense and Agriculture, concerned with applications, the research programs specific to their missions, should include all major segments of science not specifically pertinent to those missions. It should have separate bureaus or institutes with suitable directors of distinction to deal with space, atomic energy, medicine, weather, patents, science information, physical science, geology, and other recognized areas of importance. Each director should be aided by an advisory panel of experts in his area, drawn from academic, industrial and government sources.

To provide the Department of Science administrative head with broad and helpful advice it would seem reasonable to create an advisory council, which might be designated a National Science Council. Such an advisory group should not be exclusively academic but should include representation from government, science, and industry as well. Its principal responsibility would be to provide the Science Department administrative head with broad advice which might be helpful in arriving at decisions on the extent and character of support which the government should provide both to science programs in the government and, through contracts or grants-in-aid, to industry and universities—in short, the implementation of a National Science Policy.

Commission To Study Problem

These comments of mine on the creation of a Department of Science and a National Science Council have been postulations based on the needs that exist to coordinate governmental science, to create a National Science Policy, and to establish a liaison between governmental, academic, and industrial science (*1*). Before firm and thorough recommendations can be made, a commission should be established to study all of these questions very seriously. Such a commission should include representatives of government and of the academic and industrial community in both scientific and nonscientific areas. If the United States is to achieve a balance not just in its budget but also in its scientific programs, both immediate and long-range, there has to be a thoughtful and penetrating analysis of the problem. There must be the maturity of judgment and the courage of action required to change existing institutions, procedures, or philosophies where it is necessary. Where necessary there must be a facing of the problem and recommendation of essential drastic action, so as to avoid continuance of methods or actions which merely postpone the day when action must be taken.

Conclusion

The retiring presidential address before the American Association for the Advancement of Science is a personal presentation of my own ideas and is not intended to present the opinion of the association. Last year's speaker, Lawrence H. Snyder, presented a thesis on genetical concepts which were those of the speaker on that occasion. When a university professor presents a paper before this association it is taken for granted that his opinions are his own and not those of his university. In my presentation I have pressed for a more academic status for the government scientist, with opportunity to present for discussion and consideration before his fellow scientists his own concepts pertinent to science. I realize that there are many, including some members of the association's board of directors, who do not fully support or entirely share some of the ideas contained in my presentation.

In conclusion I would urge that some form of commission or study group should be established to give careful consideration to the problem of organization of science and science policy in the academic, industrial, and governmental areas of the nation, and that in this study serious consideration should be given to the following concepts.

1) There should be a regrouping of some of the government's scientific agencies or activities: either a Depart-

ment of Science, a National Science Institute, or some other coordinated structure. A well-developed coordination must be established between the regrouped combination and those scientific agencies which remain separate, so as to insure an efficient and comprehensive National Science Program.

2) There should be a realignment of the distribution methods and responsibility for support of basic research in our educational institutions, with a movement toward university grants, administered largely by a department concerned with basic research, rather than by agencies concerned with applications. This may well need to be coordinated with the growing problem of support for our advanced-education program in all areas.

3) There should be some separation of governmentally sponsored, major research institutions from our educational and industrial system, especially of those institutions which are essentially concerned with applied science. There should be a greater acceptance of the idea of operation of such institutions under an improved, directly governmental administration.

4) The liaison of scientists in government with scientists in the academic field and in industry should be represented by a National Science Council in such a manner as to be compatible with the maintenance of our broad culture and balanced development.

Note

1. The opinions presented in this article are not intended to reflect the opinions of either the National Bureau of Standards (Department of Commerce), with which I was previously affiliated, or of the Department of State.

How Our Air Force Supports Basic Research in Europe

This unusual program of military support for open research abroad has won widespread approval.

Howard J. Lewis

A visitor to the European Office of the U.S. Air Research and Development Command (EOARDC), a unit of the U.S. Air Force that occupies suites on the top three floors of the Shell Building in Brussels, Belgium, is immediately struck by the absence of three powerful military symbols: the uniform, the armed guard at the gate, and the visitor's register.

When questioned recently about this apparent anomaly, Col. Nathan L. Krisberg, commander of the European Office, replied: "It is not our intention to disguise the essentially military nature of this enterprise, but we do want to emphasize to visiting European scientists that our mission can be accomplished only through the open support of open research."

Now in its eighth year of operation, EOARDC is administering 306 research contracts, totaling some $6,591,478, with scientists in universities, research institutes, and industrial organizations in 16 countries of Western Europe and the Middle East. In the course of its development, it has worked out a procurement system which seems, on the basis of interviews with some of its contractors, to be regarded as both strict in its demands and wise in its understanding.

The European Office is, in the words of its statement of mission, "established to procure in Free Europe research and development in support of the mission of the Air Force and provide a scientific liaison fostering mutually beneficial relations between the United States and European scientific communities."

The mission of the parent ARDC is to support the conduct of basic research on behalf of the Air Force, to develop new and improved devices, processes, and techniques, and to maintain qualitative superiority of materiel. From its headquarters at Andrews Air Force Base, a few miles across the Maryland border from Washington, D.C., ARDC divides its various responsibilities among ten research centers throughout the United States, which perform laboratory and contract research, development, and testing aimed at the improvement of vehicles, weapons, and personnel training. To tap the additional and often unique scientific resources available in Western Europe, ARDC established its European Office in 1952.

The generous allocation of support to the EOARDC program is positive evidence of its success in contributing to the military mission of the U.S. Air Force. But even more significant may be the contribution of the European Office to the cause of world peace, for it has demonstrated that an intelligently administered program of international support for scientific research weaves a sturdy fabric of understanding among nations that cannot be purchased with dollars alone and can serve as a template for long-overdue civilian enterprise in this direction.

Proposals Evaluated in U.S.

European investigators in all fields of the natural sciences are urged to submit proposals to the Brussels office for the support of their research. They are promised freedom to publish results of their work in the open literature and, indeed, are urged to publish. Incoming research proposals are screened in Brussels by a crew of Air Force scientists, all experienced in laboratory or bench work, and approximately 75 percent of the proposals are routed to one or more ARDC laboratories in the United States for evaluation. If an ARDC laboratory wishes the proposed research to receive support, it must provide the funds out of its own budget and authorize their transfer to EOARDC, where the final contract will be drawn up.

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The contract will call upon the contractor—in most cases a principal investigator representing a semiautonomous university-affiliated research team—to exercise his "best efforts" in the pursuit of a specified goal. The terms, based upon previous negotiations between the principal investigator and an EOARDC procurement officer, provide for payment by the Air Force of all or part of his expenses in salaries, expendable equipment, and—in some instances—the cost of new capital equipment. In return for this Air Force outlay, the contractor is required to file quarterly reports on progress, a final report on results, and technical notes at natural intervals in the research program. The contract may also call for a strict accounting of all expended equipment; whether this is the case depends upon the type of contract instrument chosen.

Contracts with scientists in each European country are preceded by coordination which involves explanation of EOARDC's objectives and methods, first through the United States Embassy in each country, then through the local foreign office, and finally through the appropriate civilian or military agency concerned with research and development. This sometimes tortuous path is one which fortunately must be broken only once for each country. Responsibility for the maintenance of diplomatic relationships is shared between EOARDC and the United States air attachés in each country.

Responsibility for the continuing relationships with contracting scientists, however, lies solely with the commander of EOARDC. Serving under him at the present time is a staff of 48, including 19 officers. Most of the officers are in line of command, assigned to one of three directorates: Resources, Technical Operations, and Procurement.

The staff is housed in a large office building containing no other military installation, in the capital of a country in which the United States has no fighting troops. Air Research and Development Command men stationed in the Brussels office wear civilian clothes during their three or four years of duty there.

Active Contracts Listed

It is the Directorate of Technical Operations which conducts the central assignment of the office. Acting director at the time of this writing was Lt. Col. Albert C. Trakowski, Jr., also chief of the Physical Sciences Division. There are also divisions of Bio-Sciences, Aeronautics and Propulsion, and Technical Information, but, as might be expected, the physical sciences occupy most of the technical-operations personnel, and a large part of the total of authorized funds is allocated to them. A summary of active European contracts as of 1 January 1959 is given in Table 1.

Table 1. Summary of active European contracts as of 1 January 1959.

Science	Authorized funds
Biosciences	$ 941,458
Chemistry	504,915
Electronics	1,904,293
Geophysics	936,816
Materials	164,662
Materiel	90,092
Mathematics	108,017
Mechanics	538,762
Metallurgy	305,192
Physics	633,431
Propulsion	199,509
Solid-state sciences	242,131
Armament	22,200

Substantial expenditures in support of the life sciences reflect the almost single-minded zeal of Lt. Col. James P. Henry. From April 1956 to November 1958 he handled alone the assignment of technical operations in the biosciences, monitoring some 50 contracts in his field, most of which he had played a large part in originating. Working closely with an old associate, Harvey E. Savely, Jr., of the Air Force Office of Scientific Research, Henry sees in his research-support program the means for developing deeper insight into the learning process, stronger defenses against the effects of sensory deprivation in space flight, and greater understanding of the nature of environmental stress in the piloting of supersonic aircraft.

Most of his leads, Henry says, emerge from conversation with European scientists, some of whom are already under EOARDC contracts; others are offered by visiting colleagues from ARDC and the Office of Naval Research, American contractors to ARDC, and private investigators in the United States. Henry pays close attention to European scientific journals and the proceedings of appropriate meetings in Western Europe. He will grant that the program he has helped develop for the ARDC among European scientists does not cover all parts of the life-sciences spectrum, but he feels that it is his mission to develop a program which corresponds to his own highly personal assessment of the most promising fields of research for the Air Force.

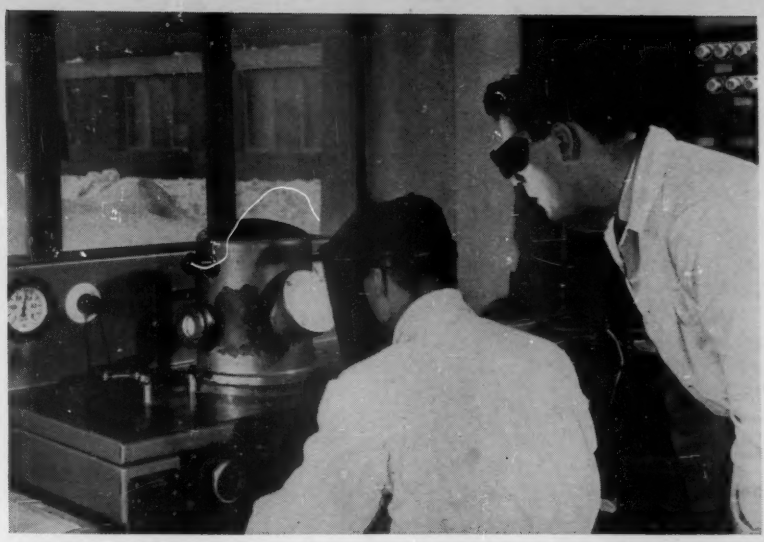
This highly motivated concentration of interest in a particular field of study has its counterparts in the Physical Sciences Division, particularly in the work of Trakowski. Organizer and first director of the Air Force's Geophysics Re-

The procurement officer and the technical project officer discuss a proposal for USAF-supported research with Prof. Pol Swings. (Left to right) Capt. Clifford C. LaPlante, Lt. Col. Albert C. Trakowski, and Prof. Swings.

search Directorate in 1946, he declares he has helped to introduce ARDC support to "every major scientific institute in Western Europe that can and is willing to contribute to our present objectives."

Although Trakowski is project officer for approximately 55 contracts at the present time, which cover a wide range of the physical sciences, he acknowledges happily that he has devoted most of his creative effort to the furtherance of fundamental research in the atmospheric sciences, to meet the needs of the Air Force for complete knowledge of its operating environment. During a recent interview, Trakowski wrote down these three principal goals: (i) to determine the electronic, nuclear, atomic, and molecular reactions and energy-transport mechanisms in the gases of the atmosphere under terrestrial, solar, and cosmic influences; (ii) to develop mathematical, hydrodynamic-thermodynamic models of the atmosphere to permit weather prediction by electronic-machine numerical computation; and (iii) to discover the physical processes underlying atmospheric water condensation and precipitation as the basis for weather modification.

Until recently, Trakowski observed, there had been a great deal of interest in supporting studies of atmospheric electricity as an avenue leading to weather control. Recently, however, ARDC research teams in the United States have shifted their emphasis to physical-chemical studies, and EOARDC has followed suit, terminating activities in the former field and encouraging investigators in the latter.



Scientists in Oslo are shown experimenting with an arc laboratory furnace. The project, called "Mechanism of the oxidation of titanium and titanium alloys," was supported by EOARDC.

European Scientists Satisfied

Although I did not have an opportunity to interview a large enough number of European scientists to guarantee complete representation, the near-unanimity evident in separate conversations with six contractors chosen at random would seem to indicate that there is a widespread feeling of good will toward EOARDC among European scientists and a general satisfaction with the latitude and flexibility of its contractual relationships.

In general, these contractors said that ARDC support enabled them to (i) pursue lines of research that would otherwise have been closed to them; (ii) train students and professional assistants who might otherwise be lost to basic research; and (iii) visit fellow investigators and their laboratories in the United States. Two individuals in this small sample expressed greatest satisfaction that their funds permitted them to hire young American scientists for work in European laboratories, thereby bringing about a day-by-day international exchange of ideas, information, and techniques, in contrast to the more sporadic exchanges made possible by publications and international conferences. It was pointed out that in some national support programs in Europe, the use of funds for the hiring of non-nationals is prohibited.

Even when hypothetical problems were introduced into the conversation, they were disposed of without any show of serious concern. It was not felt by any of those interviewed, for instance, that the present level of ARDC support could substantially influence the direction or the nature of scientific research within any particular nation, since it is relatively such a small part of the national expenditure. Nor did any of those interviewed feel that his particular line of study had been substantially deflected by its association with the quite different mission of ARDC. As one put it, "At intervals in research, there comes a time to branch out in any one of four or five different avenues of promise. We may, because we now have an idea of the kind of proposal that is attractive to the Air Force, choose one of the four or five that would not otherwise be our first choice. But they are all promising, and they are all a continuation of the fundamental research that interests us."

The scientists interviewed were equally resigned to the constant possibility of contract termination. Each said that all personnel employed on the project had been acquainted with the situation; in some instances, wages and salaries had to be increased over the local level in order to counterbalance the lack of security. All, however, expressed the belief that longer-term contracts would permit the hiring of more talented personnel at the same or even lower salaries. Similarly, although all appeared to be ready to accept, without reservation, the requirement that all capital equipment purchased under their contracts be returned to the U.S. Government, each expressed the belief that the goals of both parties would best be served if some mechanism could be developed that would permit European laboratories to gain title to such equipment. Serious consideration is now being given within the ARDC to an upward revision of time limitations on contracts and to the establishment of an authority that would allow contractors to earn title to capital equipment purchased with Air Force funds by conducting further research for a period of time to compensate the Air Force for the residual value of the instrument.

Only in two matters were there some murmurs of dissatisfaction. One contractor confessed to a lack of enthusiasm for the detailed accounting necessary to obtain reimbursement of costs

for expendable equipment. Another felt that contractors should be required to submit final reports at a logical breaking point in the research, rather than at the arbitrarily set date for termination of contract—especially, he said, in those cases in which a second contract has been granted in extension of the first.

Although an EOARDC contract can often provide a European scientist with an annual budget many times that which he could expect from his own university or government, it does not seem that the evident satisfaction with EOARDC merely represents approval of an agency that is playing Santa Claus. More credit must go to the clarity and continuity with which the several commanders of EOARDC have pursued their mission.

Early Ups and Downs

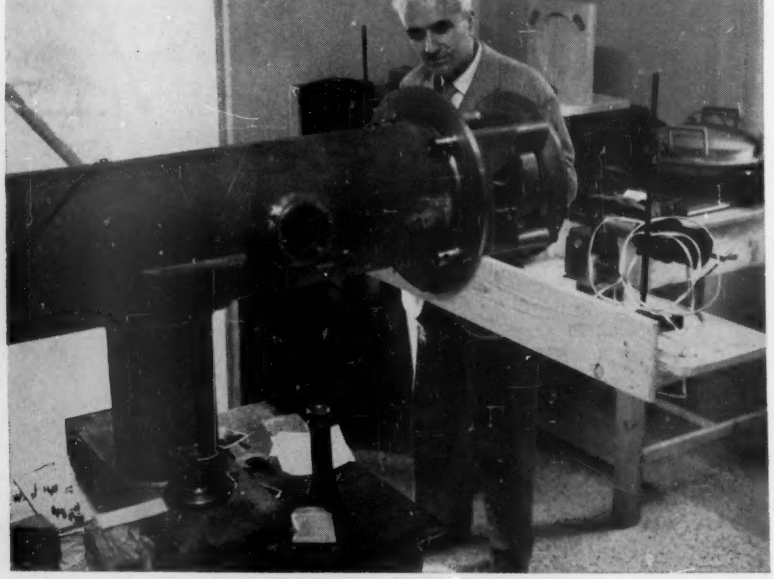
Those who were close to the initial operations of EOARDC report that at first its overtures were met with coolness. Some of the scientists originally approached indicated that their skepticism was based on a vision of Laocoön entangled in a serpentine of military red tape. But most of these fears seem to have been laid to rest.

Responsible in large part for the philosophy and initial success of the European Office was Colonel Oliver Haywood, who with the support of General Donald A. Putt drew up the initial plans that resulted in the establishment of EOARDC, in October 1952. A graduate of the U.S. Military Academy, with a Ph.D. degree earned later, Haywood had been one of the early advocates, within the military establishment, of greater participation in fundamental research, pointing out that such a relationship is essential for the rapid integration of new capabilities into any research and development program. The first expression of the Putt-Haywood collaboration had been the establishment of the Air Force Office of Scientific Research (in 1951), whose program of support for fundamental research is also within ARDC. It was after directing the successful AFOSR operation for approximately two years that Haywood proposed a European branch to advance the interests of the United States and the free world by providing a far broader support of scientific endeavor without a correspondingly greater increase in cost. Named as EOARDC's first commander was Lt. Col. Ralph Nunziato.

Within a short while after Nunziato's arrival on the European scene and after a series of interviews with leading European scientists, an operating procedure had been worked out whose basic elements obtain to this day. Pains were taken to assure the European scientific community that there was to be no restraint on the flow of information and that the main instrument of negotiation between the partners of an EOARDC contract was mutual trust and respect for the integrity of the professional scientist; the contract was to be the description of the relationship, rather than its bond. There would be no recruiting by ARDC personnel for emigration to America, nor would EOARDC allow itself to be used for this purpose by others. Inflation would be avoided by scrupulous observance of prevailing wage and salary scales. The office would endeavor to avoid saturation of any particular institution, in terms both of percentage of qualified personnel under contract and of percentage of total research budget. Only 100 copies of final reports would be required from contractors, in order that later journal publication should not be interfered with.

The EOARDC was not, however, allowed to climb steadily to its present position. Like most Air Research and Development Command units, it was adversely affected when the Command's budget was cut approximately 10 percent between fiscal years 1953 and 1954. Support for basic research was all but eliminated from the Air Force program. Haywood resigned his position and commission.

Then, from Air Force Secretary Harold E. Talbott and Lt. Gen. Thomas S. Power, came new and powerful support for a reinvigorated program. Talbott argued successfully for restoration of research funds; Power was moved to put part of the new funds into an expansion and revitalization of the ARDC's European Office. To help reestablish ties with European scientists, Power sent Brig. Gen. Don Flickinger to support the EOARDC staff in its missionary program. Command Surgeon and Director of Human Factors for ARDC and first commander of the Air Force Office of Scientific Research, Flickinger, with the staff of the European office, outlined EOARDC methods and goals, promised a new and better contract (one particularly suited to the support of fundamental research), and reemphasized the point that contract funds could be expected to cover expenses to and from



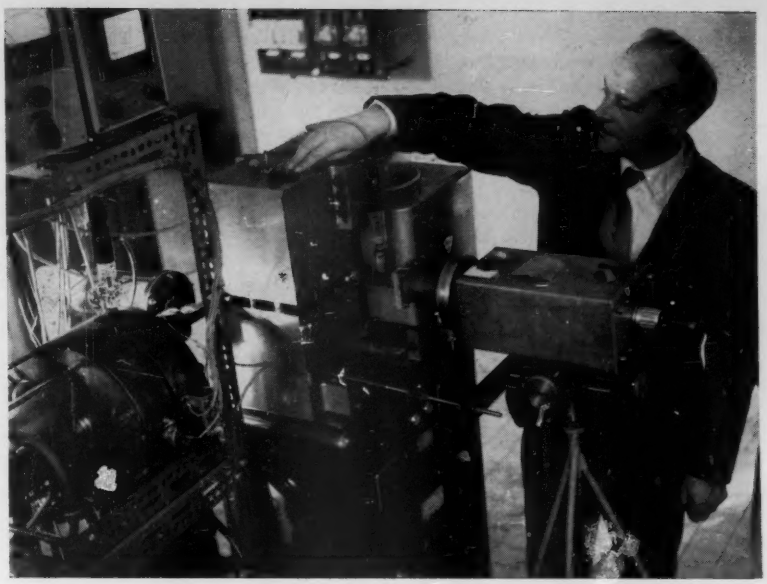
Prof. G. Righini, of the Arcetri Astrophysical Observatory, holds an EOARDC contract for research on stratification of the atmosphere. He is also studying the solar spectrum and for that purpose is building the spectroscope shown here. He obtained many of its components from dismantled optical equipment of the Italian Navy.

symposia and conferences in Europe and the United States.

The European office suffered another drastic curtailment of funds in 1957, but once more persuasive argument, by its then commander, Colonel Lee V. Gossick, and by highly placed stateside supporters, together with the timely appearance of the first Soviet artificial satellite, resulted in the speedy restoration of budgetary cuts. By the end of the 1957–58 fiscal year, 243 contracts, which totaled $3.1 million, had been written.

Part of the credit for the success of EOARDC must go to the contractual instrument itself. As explained by Captain Robert E. Rochfort, director of procurement at EOARDC, the particular contracts used for the procurement of basic research are of recent development and have been given separate listing in the Armed Services Procurement Regulations within the last few years, although the use by the Air Force of specific contracts for research came into existence before ARDC was constituted as a separate command.

But there has been even further specialization within the European context. According to Rochfort, it is the practice in the United States to use cost-reimbursable (C.R.) contracts for research, whether basic or applied. "A C.R. contract is used," he said, "when costs cannot be realistically estimated, but since most of our contract costs are generally predictable, EOARDC finds it advantageous to the contractor and the Government to use a Fixed Price [F.P.] Contract. When we run into a situation where we have both elements of costs (those that can be firmly estimated and those that cannot be), we use an F.P. Contract with a C.R. provision. Straight C.R. is also used when the situation demands." Joint support by ARDC and the scientists' home country often solves the problem of capital equipment purchases, since funds from the latter source can be used exclusively for such purchases, while ARDC funds can be devoted to salaries, wages, and the purchase of expendable equipment.



Prof. A. Van Tiggelen displays his apparatus for studying reaction kinetics of flame propagation in his laboratory at the University of Louvain—a study supported by EOARDC.

Advantages to This Country

To European scientists the program of EOARDC has meant support for research that would otherwise in many cases have been severely limited in scope or completely out of reach. What have been the rewards, so far, to the ARDC? These cannot be expressed in terms of military utility, since successful applications—if any—must remain shrouded in secrecy. Yet these rewards can be expressed generally—and often are—by EOARDC personnel. From their viewpoint, the participation of ARDC in the support of basic research has meant, first of all, that the bench scientists and technologists serving ARDC have the opportunity, almost literally, to look over the shoulder of European researchers in the pursuit of fundamental knowledge. The agreements permit the contractors to publish freely the knowledge gained under the program, but the critical lead time between research and development is sharply reduced for ARDC in two ways: Papers resulting from ARDC-supported research usually do not appear in the contractor's journal of choice for 6 to 18 months after they are submitted as final reports to the supporting development center, and the fact that the center has had continuing access to quarterly status reports enables it to utilize the new knowledge more quickly. Equally important are the personal contacts that are developed between ARDC personnel and the European scientific community, often extending far beyond the termination of the contract.

In addition, EOARDC has developed with the sister services of the Air Force what may well be a unique example of interservice cooperation. The success of the ARDC office in Brussels prompted, in part, the establishment of two similar groups, the Naval European Research Contracts Program and an Army European Research Office, in 1956. Representatives from the three offices meet quarterly to exchange information and coordinate efforts. Supplementing these quarterly meetings is a monthly exchange of information on proposals submitted to each office and notifications of approval. The quarterly status reports required by EOARDC contracts are distributed similarly to the Army and Navy groups.

Coordination at the operational level seems more attractive to those connected with the EOARDC operation than any suggestion of a merger of all offshore research support within a single Department of Defense agency. Flickinger pointed out that interdepartmental control would seriously attenuate the close relationship that has been developed between ARDC researchers and European contractors.

There remains one large consideration. Members of other military-oriented research organizations have questioned the wisdom of offering support to European investigators when men of equal talent may be available within the United States. Although many of EOARDC's contractors have

unique capabilities, it has not been stated that uniqueness must be established before individual proposals may receive supporting funds.

I do not consider the answer to that problem to be within the scope of this article. Nor was any answer demanded of the EOARDC staff. Yet one statement from Krisberg, made in another context, might be considered at least a partial answer. In a conversation with me one afternoon in Brussels, he said, "It must now be obvious to all that science is today one of the major battlefields in the cold war. It seems equally obvious to us that in this war, the brain power of the Western World is the maximum weapon. We feel that the European Office can, through initiative and understanding, contribute to a unity of purpose."

Louis N. Ridenour, Physicist and Administrator

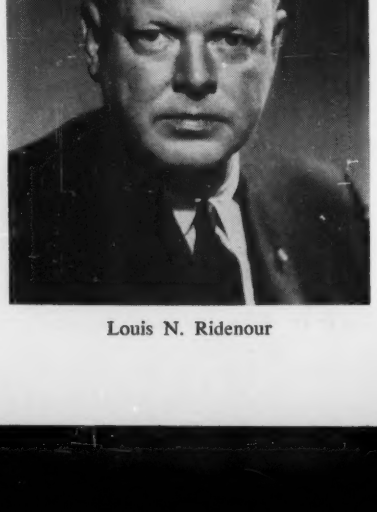
When Louis N. Ridenour died on 21 May 1959, the scientific community lost a remarkable physicist and scientific administrator who had an uncanny ability to sense the areas in which a scientific or a technological "harvest" is likely. He devoted himself to helping achieve and exploit such break-throughs. This he did as a working physicist in his early years. In his later years he accomplished his goal as dean of the Graduate College at the University of Illinois, as chief scientist of the Air Force, as an executive with International Telemeter Corporation, and, finally, with Lockheed Aircraft Corporation.

He was born on 1 November 1911, in Montclair, New Jersey. He took a B.S. degree in physics at the University of Chicago, where he was the editor of the *Daily Maroon*, the student newspaper, in his senior year. This experience undoubtedly was closely coupled with his unusual facility as a writer of both technical material and fiction.

Ridenour received his Ph.D. degree in physics at California Institute of Technology, under Lauritsen, in 1935 and went to the Institute for Advanced Study, Princeton, New Jersey, in the fall of that year. He was to be an assistant to Fermi. Although the latter did not come to Princeton, Ridenour stayed as an instructor in physics at Princeton University until the fall of 1938.

At that time he joined Harnwell, the head of the physics department at the University of Pennsylvania. Since he felt that nuclear physics was badly in need of precise measurements, he undertook the construction of an electrostatic accelerator with accurate high-voltage control. This work was interrupted when he was called to the Radiation Laboratory at Massachusetts Institute of Technology in 1941. He tried to continue work on the machine by long distance and by making periodic visits to the University of Pennsylvania. His plans for this work never came to fruition, since the electrostatic generator was destroyed by fire soon after being placed in operation.

The years 1941–46 were crowded with great events for many physicists, particularly for Ridenour. He played an important role in determining which of the devices being developed at the Radiation Laboratory could be used effectively, in influencing the development of such devices, in persuading those involved in application to use them, and finally in working near the front of combat in order to make certain that the devices were used properly. He possessed an almost unique ability to influence all phases of a situation.



Louis N. Ridenour

He was appointed editor of the *Radiation Laboratory Technical Series* in 1945. The series consists of a set of 28 volumes which describe the topic of radar as it stood at the end of the war. Concerning this effort, F. W. Loomis [*Phys. Today* **12**, 18 (Sept. 1959)] made the following statement: "Rarely has a man so perfectly fitted a job. It called out all his qualities—his facility in writing, his talent for assembling and leading a group, his own encyclopedic knowledge of radar and electronics, plus his quick ability to learn and understand what he didn't already know; and especially his persuasiveness and influence in high military quarters, needed to overcome the timidity of the security-conscious bureaucrats. . . . It is used by all the engineers in the now huge radar industry and serves to educate the new generations as they come along."

Ridenour became dean of the Graduate College of the University of Illinois in 1947 after returning to the University of Pennsylvania for one year. During his three-year tenure as dean, he played a major role in initiating new programs and study groups at the University of Illinois. Among them are the Control Systems Laboratory, the Digital Computer Laboratory, the microbiology group under Luria and Spiegelman, the Radio Carbon Laboratory, and the solid-state group under Seitz. His enthusiasm, drive, and administrative wisdom contributed much to the successes these enterprises achieved.

In 1949–50 he served as chairman of an *ad hoc* committee to survey research and development in the Air Force. The report of this committee, the Ridenour Report, recommended the formation of the Air Research and De-

velopment Command and the establishment of the post of Deputy Chief of Staff for Development. In the summer of 1950 Ridenour became chief scientist of the Air Force in order to help implement the recommendations of this report. During his tenure in that position he sponsored the establishment of the Massachusetts Institute of Technology's Lincoln Laboratory, which developed the SAGE air defense system and the Dew Line.

He never returned to the University of Illinois because his younger daughter became seriously ill. Instead, he took a position as vice president of International Telemeter Corporation in Los Angeles. While with that company he endeavored to develop pay-as-you-go television devices and computer components. He is largely responsible for the formation of Telemeter-Magnetics.

In 1955 he joined the Lockheed Aircraft Corporation. There he was much concerned with the gathering of personnel for the research division of the Missile Systems Division. He rose through a series of positions and responsibilities; just two months before his death he was named vice president of the company and general manager of the new Electronics and Avionics Division.

Ridenour was awarded the President's Medal for Merit and the Bronze Star for his services during the war. He was awarded, posthumously, the U.S. Air Force's highest peacetime decoration, the Exceptional Civilian Service Medal.

He is survived by his widow, the former Gretchen Hinkley Kramer, and his two daughters, Eleanor and Nancy, as well as his parents, Louis N. and Clare Ridenour.

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Science in the News

Next Few Years Said To Promise Solution of Antarctic Research Program's Recruiting Problems

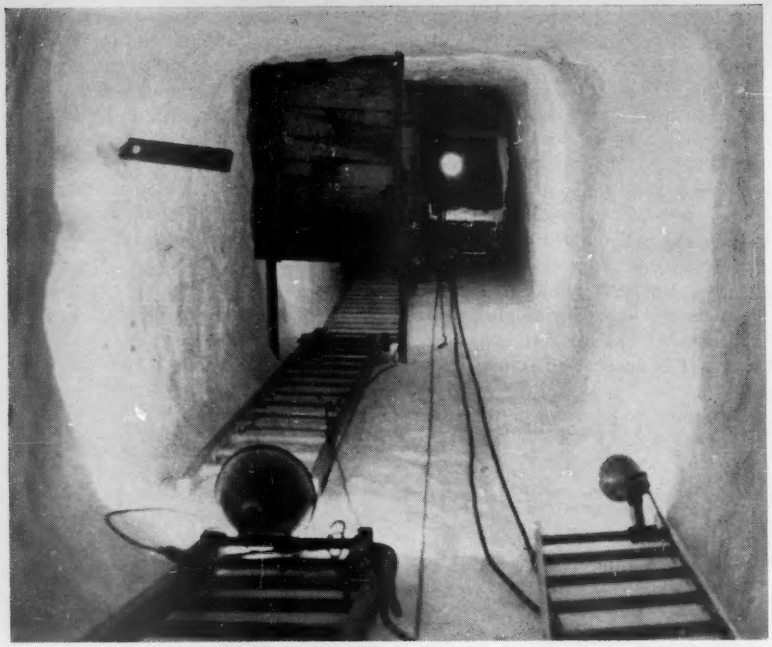
Recruiting problems, which are adversely affecting this country's antarctic research program, will probably be solved within the next few years, according to T. O. Jones, the National Science Foundation official who directs the program. Jones, who recently returned from a trip to the U.S. stations in Antarctica, believes that policies now being instituted will attract enough scientists to antarctic research to leave NSF, which supports such work, only the problem of selecting from among them those whose projects will contribute to a balanced program. The current program, which is now under way with the onset of the antarctic summer, suffers, according to observers, from a lack of balance between scientists and technicians. This situation, in which the technicians heavily outnumber the scientists, was suited to the activities of the International Geophysical Year, but the relative numbers now must be reversed, Jones feels, if a long-term antarctic program is to be carried out and is to produce significant results. The foundation hopes to attract people to the Antarctic who will be able to pursue individual research projects in addition to making the routine readings and measurements for programs that are hold-overs from the International Geophysical Year.

Three developments, among others, will work toward a solution of the problem, Jones feels. The first is a growing awareness on the part of American scientists of the possibilities for research offered by the opening up of Antarctica. The second is the increasing availability of adequate scientific facilities and transportation in the Antarctic. The third is the possibility that special institutes can be established in the United States for workers in the academic field for whom research in Antarctica involves absence from universities and colleges during the academic year.

Efforts by Jones and his staff to acquaint American scientists, particularly those in universities, with research possibilities in Antarctica are beginning to show results, according to foundation officials. More numerous and more varied proposals are being received at NSF headquarters in Washington. Scientists who have returned to their universities from work on the Antarctic continent during the IGY or post-IGY period are discussing this work with their colleagues and graduate students, with the result that men with the training the program needs are becoming interested. Jones himself has taken a number of university scientists to the continent to see the work that is being done and to visualize what can be done. This interest, Jones feels, is a seed which in time will produce projects that will fill out the antarctic research program—a program that is no longer just one aspect of a world-wide geophysical program but is slowly achieving a new status as a complete and balanced undertaking in its own right.

The new and well-equipped biological laboratory at the McMurdo Sound Naval Air Facility is one of the evidences that the needed equipment for scientific work is now becoming available on the Antarctic continent. It provides facilities such as are found in the laboratory of a small college, and plans are under way to enlarge it. New living quarters will soon be built for scientific personnel at this base. At the more isolated stations, such as the Byrd and the Scott-Amundson stations at the South Pole, electronic and photographic equipment is available for studies of the ionosphere and of aurora and for work in the fields of geomagnetism, seismology, meteorology, and other disciplines. Plans are under way to transform the antarctic bases which were built for temporary use during the IGY into permanent stations.

January 1960 will mark the beginning of a new program in transportation that promises to be a great aid to research in the Antarctic. Ski-equipped



View up the shaft of a 100-foot snow mine dug near the Wilkes Station at 66° 15′ S, 110° 3′ E, in the Australian quadrant in Antarctica. Surfaces revealed in this way can be read much as the rings on a tree are read and provide valuable data on past weather conditions and on types and accumulations of snow. [Official U.S. Navy photo]

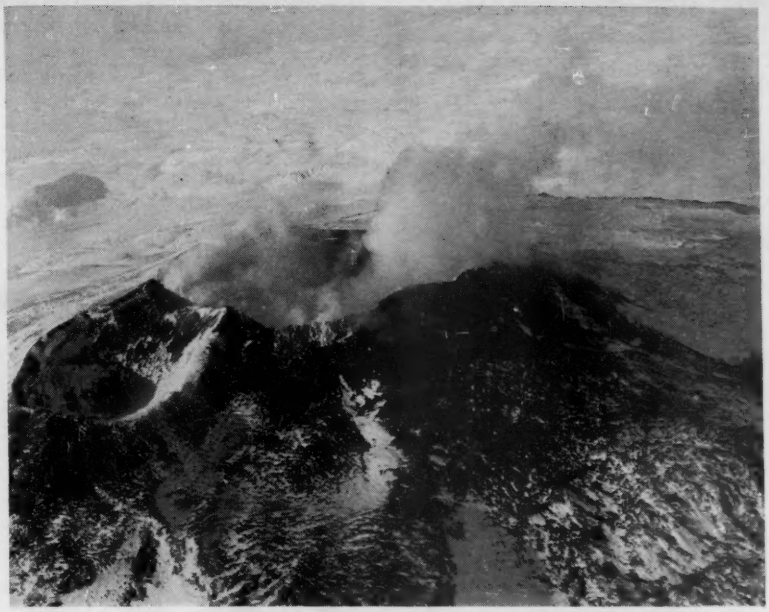
Location of some of the United States bases used in the antarctic research program. The off-continent support base is in Christchurch, New Zealand, 2140 miles from the Naval Air Facility, located on Ross Island at the edge of the Ross Ice Shelf. A new airstrip (shown in inset) is under construction on the permanent ice of the shelf.

Air Force Hercules transports will begin preliminary resupply work, operating from a landing strip now being prepared on the permanent ice of the Ross Ice Shelf. Use of these planes, with their great range and great load capacities, is expected to make possible more regular supply operations and better communication with the inland stations. Scientific personnel will be able to reach the stations just in time to begin their work. Under present conditions transportation to these bases is so irregular that, for example, men planning studies of aurora find themselves at inland stations 3 months before the antarctic night and the aurora begin. Others, planning to study the nesting behavior of the penguins, arrive at the rookeries after the nesting has begun. Although weather always has been a factor in such misfortunes and always will be, regardless of the type of aircraft used, the new transports promise to cut down on waste time and effort.

Polar Institutes

Spring in Antarctica is fall in the United States. A university-affiliated scientist who wants to work on the Antarctic continent during the 6 months of summer light must leave for the area at just the time that his university colleagues are resuming their classes. His research project may require two consecutive antarctic summers for completion—two full academic years. Many university scientists are reluctant to give up this much time for work on "the ice."

In an interview in Christchurch, New Zealand, the staging area for the U.S. program in Antarctica, Jones explained his views on a possible solution to this problem. He believes that special academic institutions can be set up in the United States for people who spend a number of years in antarctic research. A number of universities, he feels, are in a position to set up polar research and educational centers. Such centers would have five major functions. They would have academic schedules that conform to the antarctic seasons—that is, the main sessions would be held during the U.S. summer. They would attract young people who, both because of concern for science and a desire to travel, are interested in working in Antarctica. They would be active in the development of equipment and materials for work under high-latitude conditions. They would promote cooperation among the various disciplines



Mount Erebus, believed to be the only active volcano on the Antarctic continent, is located near the U.S. Naval Air Facility on McMurdo Sound, from which U.S. scientific bases in the interior of the continent are supported. [Official U.S. Navy photo]

concerned with research in these latitudes. And they would take on the function of data reduction for information obtained through antarctic research.

Although such centers may in time provide the answer for the academic scientist, temporary solutions are needed. Recent visitors to Antarctica report that such solutions are being developed. At the new biological laboratory at McMurdo Sound a series of seminars, conducted by the scientists who have gained experience in antarctic research, has been under way for some time. Field workers who visit the laboratory after completing a traverse or after a year's work at one of the inland stations give lectures on their work. This program gives promise, according to Jones, of developing further in the coming years. With the increase in the number of university people participating in the antarctic program, the academic approach is more and more evident. At the airlines terminal in Christchurch one can occasionally see an investigator conducting an informal seminar with his graduate students before their plane departs for Antarctica. Jones feels that an investigator and three or four graduate students constitute the ideal unit for antarctic research. Such a group could divide its time between field work on the continent during the antarctic summers and formal academic work in the United States during the remainder of the year.

The interest expressed by scientists now working in the Antarctic seems to promise that the "laboratory continent," as Jones describes it, will become increasingly attractive as an area for scientific research. Certainly the expansion of facilities and the recent conclusion of the Antarctic treaty, which reserves the continent for peaceful purposes, particularly scientific work, point in this direction.

Suspension of Test Ban Talks in Geneva Leaves Many Problems Unresolved

Problems that were revealed last month during sessions of the scientific committee at the atomic test ban talks in Geneva threaten to increase the difficulties of the group discussing political questions, which will resume its work on 12 January. The committee of scientists from the United States, Britain, and Russia, which was studying the technical aspects of a control system to police the ban, submitted a report which indicates that no agreement has been reached on the major issue of what criteria should be used to determine whether tremors recorded by control posts are caused by natural earthquakes or underground nuclear explosions. Agreement was reported, however, on improvements in instrumentation to be used at the posts.

The main obstacles to agreement came in discussions of the latest United States data concerning the difficulty of distinguishing earthquakes from nuclear tests and the possibility that tests could be deliberately disguised to make their identification more difficult. These data, which had been submitted to the scientific committee last January, were not examined until November, because the Soviet Union refused to permit their joint consideration.

The slow progress of the talks has raised the question of what action the U.S. will take in regard to the test ban that was due to end on 31 December. Before the talks opened last November, John A. McCone, chairman of the Atomic Energy Commission, said that he believed that this country's voluntary suspension should be extended only on a week-to-week basis after its formal expiration. This extension, he said, should be made on the basis of the promise shown at the test ban talks. How this general principle will be applied now that the talks are temporarily ended is yet to be seen. The Soviet Union has said that it will continue its ban as long as the U.S. refrains from nuclear testing.

Hopes Were High

During the period of more than a month that the talks were under way, considerable hope was expressed by both the conferees and government officials that general agreement might be forthcoming. At his news conference 2 December, President Eisenhower said that he was more optimistic about the talks than he had been ". . . a few months back when it looked like they were going to be completely abandoned." After the suspension of talks, however, State Department spokesman Lincoln White said that the department was "greatly disappointed" that the meeting has concluded with only "minimal agreements." He also said that he "could not answer" as to whether the United States would refrain from nuclear testing until the resumption of the political talks on 12 January.

Observers here suggest that the United States may set a cut-off date for the talks to avoid an indefinite con-

tinuation of the test ban. Some U.S. officials are reported to be chafing under the ban which was undertaken in October 1958 to create a favorable atmosphere for the talks. Particularly distressing to these officials, according to reports, is the scientific evidence that indicates that the Soviets, at least theoretically, could be conducting underground tests without any risk of detection. Behind this concern is a report recently made public by the Government indicating that an underground explosion of 300-kilotons could be successfully masked by use of the "big hole" method of muffling the force of an explosion. The report says that if an underground chamber were large enough, the force of an explosion would be contained within it. The report, which was prepared by the RAND Corporation, an Air Force-sponsored research organization, says that, under certain conditions, a 300-kiloton explosion could not be distinguished from a natural earthquake by the detection net that is now proposed for policing the test ban. Explosions in the range of 100-kilotons could not be detected at all by the proposed system, according to the report.

Soviets Ridicule Report

The Soviet view of the data presented by the report was unusually blunt. "The Soviet experts submit that here their U.S. colleagues are on the brink of absurdity . . . their criteria would leave under suspicion the overwhelming majority of earthquakes registered by the control system." Citing what it calls errors in the data with which the United States supports its case, the report went on to say: "The Soviet experts therefore cannot regard these shortcomings as resulting from carelessness or coincidence, and have come to the conclusion that there has been tendentious use of one-sidedly developed material for the purpose of undermining confidence in the control system, whose basic characteristics were determined by the 1958 Geneva conference of experts."

On 28 December Senator Albert Gore urged the Administration to abstain for 3 years from conducting nuclear tests in the atmosphere but to make no commitment about not resuming them underground. Gore is a member of the Joint Congressional Committee on Atomic Energy. His suggestion was essentially the same as the one he made November 1958.

$300 Million Increase in Space Agency Budget Held Inadequate for Major Acceleration of Program

No new major effort to speed up this country's space program in fiscal year 1961 is planned, according to a newspaper report indicating that the Administration has decide to ask Congress for $800 million to support the National Aeronautics and Space Administration. The figure, which is $300 million more than the agency's current budget, was reported in the *New York Times*. Official comment on NASA's proposed budget will not be available until late next month when the national budget is published. A good part of the new funds will be taken up by the newly assigned Saturn booster program with the result that NASA's effort in its other programs will be carried on at about the same rate as last year. This, in effect, indicates that a decision has been taken against accelerating the U.S. space program to match or overtake the Soviet Union in this field. Another indication that such a decision might be forthcoming can be seen, observers suggest, in a speech given early in December by George Kistiakowsky, special assistant to the President for science and technology. Speaking before the American Institute of Chemical Engineers, Kistiakowsky said: "If one separates civilian space science and technology from military missiles, . . . one wonders whether our insistence on superiority in space is of overriding importance." Earlier in his talk he had cited the Administration's argument that the larger size of the Soviet missiles gave them no advantage for strictly military applications.

Substantial Cut Reported

The budget originally requested by the space agency for its activities was cut substantially by the Bureau of the Budget, according to the news report. No definite information on the size of this original request or on the bureau's action will be available until the Congress begins its authorization hearings. Both the Aeronautical and Space Sciences Committee of the Senate and the Science and Astronautics Committee of the House of Representatives must pass on the Administration's request. Last year, congressional action cut the amount the Administration had requested by $30 million. Also last year, T. Keith Glennan, the administrator of NASA, suggested that new programs would require a budget of approximately $100 million. Despite the Congress' action last year, there is reason to believe that the administrator will receive new support from it in his efforts to gain more funds. A number of Soviet successes, particularly the two moon probes of last fall, are expected to weigh heavily in the committees' deliberations. Another consideration may be the widely publicized fact that after the failure of an Atlas firing just before Thanksgiving no second attempt could be made because there were no more Atlases available.

World Forestry Congress Being Organized by United States

The fifth World Forestry Congress is to be held at the University of Washington, Seattle, 29 August–10 September 1960. The United States Government is host to this congress. The Food and Agriculture Organization of the United Nations and other agencies are cooperating in its preparation.

This is the first such congress to be held in the Western Hemisphere. The earlier World Forestry Congresses were held in Rome (1926), Budapest (1936), Helsinki (1949), and Dehra Dun, India (1954).

Although the congress is an official one, and many countries will name official delegations, participation will be open to any individual or technician with interests in this field upon application to the Organizing Committee. Such individuals may attend without special invitation.

Some 80 governments within the United Nations have been invited to take part in the congress. There are indications that it may be the largest World Forestry Congress ever held, with 1500 to 2000 participants from 55 to 60 countries.

Responsibility for making the necessary preparations has been vested in a 44-man organizing committee appointed by the Secretary of State. This committee includes representatives from interested federal departments, forestry schools, forestry associations and societies, conservationists, and forest industrialists. The chief of the Forest Service, Richard E. McArdle, has been named chairman of the organizing committee.

The committee has delegated responsibility for detailed preparations to seven working committees. Their ac-

tivities are under the continuing direction of an executive committee, consisting of the chairmen of the seven committees plus key representatives in the Washington, D.C., area. Some of these committees are quite large; for example, the program committee has 79 members and the tours committee, 66. In all, more than 270 American foresters and individuals engaged in forest industries are now helping in the preparations for the congress.

Some 13,000 to 14,000 copies of a preliminary announcement, in five languages, have been distributed to a special mailing list of key forestry agencies and institutions throughout the world. The foreign mailing list now contains more than 1800 names, about six times as many as any previous list. A more detailed information guide covering application for membership, preparation of program papers, plans for programs and tours, and so on, will be issued early in 1960. Meanwhile inquiries concerning the congress may be made to I. T. Haig, Executive Secretary, Organizing Committee, Fifth World Forestry Congress, Department of State, Washington 25, D.C.

Educators Named To Administer National Aptitude Census among High-School Students

The local officials who will administer Project Talent—a plan to test the aptitudes and abilities of a 5-percent sample of U.S. high-school students—were named recently by the administrator of the project, John C. Flanagan of the University of Pittsburgh. The 90 men, most of whom are educators or administrators at colleges and universities throughout the country, are now beginning to arrange with local school officials for the administering of the 2-day battery of tests next March. About half a million students in approximately 14,000 secondary schools will be tested during the study. One of the program's objectives is to take an accurate inventory of the talents of the nation's secondary-school students. A second, and longer range, objective is to correlate test scores of individuals with their subsequent histories, and thus to provide information for use in school counseling. A third objective is to evaluate the effects of such educational practices as accelerated programs for gifted students.

The tests will seek to assess not only a student's aptitudes but also his interests, personality, and achievements, especially in reading and mathematics. In addition, a student will be asked questions about his aspirations, family and community background, and health. Also planned are follow-up studies at intervals up to 20 years from the time of the test. The project, which is financed by the U.S. Office of Education and other government agencies, has been timed to coincide with the 1960 population census. It will be carried out in public, private, and parochial schools in both rural and urban areas. The names of the schools will be announced later this month.

Biological Data Handbook

On 1 September 1959, the *Handbook of Biological Data* office, henceforth to be known as the Office of Biological Handbooks, was transferred from the National Academy of Sciences–National Research Council to the Federation of American Societies for Experimental Biology. A new Committee on Biological Handbooks, responsible for policy, has been selected; its chairman is Raymund L. Zwemer of the science adviser's office in the Department of State.

The committee has approved the preparation of a tabular compilation on "Composition of Blood and Body Fluids," which is to be completed in the autumn of 1960. Then work will begin on a handbook on growth. Additional projects were discussed at the first meeting of the committee, which was held on 16 November in the *Handbook* office in the Dupont Circle Building, 1346 Connecticut Ave., NW, Washington, D.C.

Navy Surveys Basic Research

A Navy study of basic research that was released last fall outlines the characteristics of such research in industry, government, and universities and reports some significant facts. After emphasizing that basic research investigators "are exceedingly rare in number," the report points out that most of them have doctoral degrees, although only 2 percent of the nation's college graduates continue their education through the doctoral level. Of this 2 percent, only about one in five remains in basic research work. Studies indicate that not more than half of these have the outstanding talent necessary for the creative work of basic research and that this half produces 80 percent of the scientific output. At present the United States has about 27,000 basic research scientists, according to the survey; therefore, about 13,500 investigators are producing the country's principal research results.

The leading corporations in the United States are investing an increasingly large proportion of their research and development budgets in basic research, in some cases as much as 20 percent. The report states that the majority of the research directors interviewed for the study thought that some 15 to 20 percent of the Navy's research and development budget should be allocated for basic research. At present the ratio is 6 to 8 percent.

The 2-year study was conducted for the Naval Research Advisory Committee by Arthur D. Little, Inc., of Cambridge, Mass. Headed by Guy Suits, vice president and director of research for the General Electric Company, the committee is the Navy's top advisory group on research. Copies of the two-volume survey may be obtained from the Office of Technical Services, U.S. Department of Commerce, Washington 25, D.C.

Summer Institutes Announced for High-School and College Teachers

Funds will be available in 1960 to help about 18,000 high-school and college teachers of science, mathematics, and engineering to participate in 379 summer institutes sponsored by the National Science Foundation. Grants totaling more than $21 million will be awarded to support the institute program in 265 educational institutions located in the 50 states, the District of Columbia, and Puerto Rico.

Some 316 of the institutes will be open to high-school teachers only, 37 will be for college teachers only, 24 will be for both high-school and college teachers, and two will be for technical-institute personnel. Approximately 16,000 high-school teachers and 2000 college teachers will participate, with the aid of NSF.

The success of previous summer institutes has contributed greatly to the growth of the program. The first two summer institutes supported by the National Science Foundation were held in 1953. The number has grown each

summer: 125 in 1958, 350 in 1959, and 379 in 1960.

Seventeen institutes offering courses in radiation biology for high-school teachers, and five similar institutes for teachers in small colleges, are being jointly sponsored by the foundation and the Atomic Energy Commission, as are three institutes in isotope technology for college teachers.

The number of teachers who will receive financial support in each of the 379 institutes will average nearly 50 and will vary from 15 to more than 100. Tuition and fees will be paid for these teachers. They will also receive stipends of not more than $75 per week for the duration of the institute, plus allowances for travel and dependents. The institutes will vary in length from 4 to 12 weeks.

Participants will be chosen by the institutes themselves, not by the National Science Foundation. Inquiries should be addressed to directors of the individual institutes, who are named in a list that has been prepared by the foundation.

News Briefs

The Public Health Service has announced the transfer of its Division of International Health from the Bureau of State Services to the Office of the Surgeon General. H. van Zile Hyde, assistant to the surgeon general for international affairs, will be chief of the division, which he also headed from 1955 to 1958. Horace DeLien, who has served as chief of the division since September 1958, has been assigned to the American Embassy in Paris as medical officer in charge of quarantine activities, European area.

The division's international education and exchange program will remain in the Bureau of State Services as part of the Division of General Health Services, which administers the Public Health Service's training program for public health workers in this country.

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Regular exchanges between lecturers and research workers of Canada and the Soviet Union are to start next year, according to the official Soviet news agency Tass.

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The National Bureau of Standards is expanding its low-temperature research in order to increase the precision of temperature calibrations in the range from 90° down to 20°K, and to provide a calibration service covering temperatures from 20° down to 2°K. Steps will also be taken toward establishing fixed points and extending the International Temperature Scale below 90°K, where there is now no international agreement.

Scientists in the News

Wallace R. Brode, science adviser to the Secretary of State, has won the American Chemical Society's 1960 Priestley Medal—the highest honor in American chemistry. The medal will be presented at the American Chemical Society's 137th national meeting in Cleveland in April.

Michael Polanyi of Oxford University has been selected as the 1959 winner of the Lecomte du Noüy Award. The award was given in recognition of Polanyi's books, *Personal Knowledge* and *The Study of Man.*

Harold S. Morton and **Eugene L. Nooker,** staff members of the Applied Physics Laboratory of Johns Hopkins University, each received the Navy's Distinguished Public Service Award, presented by Rear Admiral M. H. Hubbard, chief of the Navy Bureau of Ordnance, at ceremonies on 23 November. They were cited for their contributions in the field of missile warheads.

Albert M. Potts, formerly at Western Reserve University, has been appointed professor of ophthalmology in the department of surgery and director of research in ophthalmology at the University of Chicago.

William T. Marshall, Regius professor of civil engineering at the University of Glasgow, has been appointed visiting professor in civil engineering at Northwestern University for a 9-month period beginning 1 January.

J. Herbert Taylor, professor of cell biology, Columbia University, will discuss nucleic acid synthesis and chromosome duplication as a Sigma Xi national lecturer at a number of colleges and universities during January 1960.

Herbert S. Goldberg, on sabbatical leave from his duties in the department of microbiology, University of Missouri, is doing research at the Low Temperature Research Station, University of Cambridge, England.

Joseph Chatt of the Akers Research Laboratory of Imperial Chemical Industries Ltd., England, has been appointed distinguished visiting professor in the department of chemistry of Pennsylvania State University for the spring semester of 1960. He will lecture on the chemistry of organometallic compounds, carbonyl, hydrocarbon and hydrido complexes, and the coordination compounds of the tertiary organic phosphines.

Robert B. Woodward, Morris Loeb professor of chemistry at Harvard, has been awarded the Davy Medal of England's Royal Society. The medal is awarded annually for "the most important discovery in chemistry in Europe or Anglo-America."

Recent Deaths

John Anderson, Altadena, Calif.; 83; astronomer at California Institute of Technology who was executive officer of the institute's observatory council, 1928–48; supervised construction of the 200-inch Hale telescope at Mount Palomar; 2 Dec.

Arpad Berczeller, West New Brighton, N.Y.; 55; head of the bacteriology department of Sea View Hospital, West New Brighton, and specialist in tuberculosis drugs; prewar head of the department of bacteriology at the Pasteur Institute in Paris; 28 Nov.

Janet W. Mackie, Washington, D.C.; 66; specialist in tropical medicine who retired last year from the U.S. Public Health Service; taught at the Wake Forest Medical School; 24 Nov.

Charles C. Macklin, London, Ontario; 76; retired professor of histology and embryology at the University of Western Ontario Medical School.

Walter J. Murphy, Washington, D.C.; 60; editorial director of the American Chemical Society's journals in applied chemistry and director of the society's news service; 26 Nov.

Harold S. Palmer, Honolulu, Hawaii; 69; professor emeritus of geology at the University of Hawaii; 24 Oct.

John V. Starr, Cranford, N.J.; 57; chemist and management official with the Esso Standard Oil Company for 31 years; 6 Dec.

Richey L. Waugh, Arlington, Mass.; 71; former chief of surgical service of the U.S. Public Health Service Hospital, Boston, and former teacher of orthopedics at Tufts University Medical School; 24 Nov.

Book Reviews

Handbook of Physiology. A critical, comprehensive presentation of physiological knowledge and concepts. vol. 1, section 1, *Neurophysiology*. John Field, editor-in-chief. H. W. Magoun, section editor. American Physiological Society, Washington, D.C., 1959. xiii + 779 pp. Illus. $22.

During the past two decades physiological knowledge, with the support of government and private agencies, has expanded at a progressively accelerating rate. The vast bulk of the current literature and the complexities of modern concepts make it increasingly difficult for the teacher, graduate student, and beginning investigator either to keep abreast of major advances or to begin a penetration in depth of a single field. If this new knowledge is to be useful to those who are not specialists, it must be collected, selected, and systematized. It must be fitted into the context of accepted fact and placed in proper perspective.

The handbooks of normal and pathological physiology, edited by Bethe and Roger in the 1930's, admirably fulfilled this need in the era just preceding the last war. Recognizing the need for an up-to-date compendium of physiological fact and theory, the board of publication trustees of the American Physiological Society has initiated the *Handbook of Physiology*, with John Field as editor-in-chief and Victor Hall as managing editor. *Handbooks* covering all phases of physiology are to be published during the next 5 to 10 years. Each will be planned and edited by a section editor, recognized as an authority in his specific field. It is anticipated that each *Handbook* will be revised about once every 10 years.

In no phase of physiology has the postwar development of fact and concept been greater than in neurophysiology. It is, therefore, appropriate that the first section of the *Handbook of Physiology* should be devoted to this subject. Volume 1 is the first of a three-volume series to be edited by Horace Magoun; the other volumes are scheduled for publication toward the close of 1959 and early in 1960. Comparable volumes on circulation are in preparation under the section editorship of William Hamilton and still others are in the initial stages of planning.

Neurophysiology, volume 1, consists of 31 chapters by some 35 contributors, each of whom is a recognized authority in his field. Fifteen of the chapters were written by scientists from abroad, 16 by North Americans.

The book is divided into five sections: a historical review, neuron physiology, brain potentials and rhythms, sensory mechanisms, and vision. With the exception of Mary Brazier's historical review, each section has an introductory chapter by an outstanding senior authority. Thus neuron physiology is introduced by J. C. Eccles, brain potentials by A. Fessard, sensory mechanisms by E. D. Adrian, and vision by H. K. Hartline. Both the character and the scope of these introductory chapters differ. That of Eccles is supplementary to the content of its section, and it expands on certain concepts given relatively brief treatment or omitted from consideration. Others serve more as prefaces that place the content of succeeding chapters in perspective.

The historical chapter by Mary Brazier begins with the concepts of Aristotle and Galen and continues through those of Sherrington, Horsley, Berger, Sechenov, and Pavlov. As she points out, scientific neurophysiology has, in large measure, developed since 1600. Her review is, therefore, devoted mainly to the contributions of scientists of the last three centuries. The chapter is illustrated nicely, although the off-center placement of figures and legends, too large for single-column reproduction, does not appeal to my esthetic sense. This brief and general treatment of the historical development of neurophysiology is supplemented in succeeding chapters by a more detailed consideration of past investigators' contributions to the present body of knowledge in each field.

The chapter titles of each section of volume 1 have been chosen to provide broad coverage. The authors of these chapters are eminent scientists recognized for their investigative contributions and fitted to write authoritative summaries. The section on neuron physiology includes chapters on conduction of the nerve impulse (I. Tasaki), initiation of impulses at receptors (J. A. B. Gray), synaptic and ephaptic transmission (H. Grundfest), neuromuscular transmission (P. Fatt), autonomic transmission (U. S. von Euler), and neuromuscular transmission in invertebrates (E. J. Furshpan). The section on brain potentials and rhythms includes chapters on single unit activity (K. Frank), intrinsic rhythms (W. G. Walter), evoked potentials (H. T. Chang), d-c potentials of the cortex (J. L. O'Leary and S. Goldring), and the pathophysiology of epileptic seizures (H. Gastaut and M. Fisher-Williams). The section on sensory mechanisms includes chapters on receptors in lower forms (H. Autrum), touch and kinesthesis (J. E. Rose and V. B. Mountcastle), thermal sensation (Y. Zotterman), pain (W. H. Sweet), taste (C. Pfaffman), smell (W. R. Adey), vestibular mechanisms (B. E. Gernandt), auditory receptors (H. Davis), and central auditory mechanisms (H. W. Ades). The final section on visual mechanisms includes chapters on photosensitivity in invertebrates (L. J. and M. Milne), the image-forming mechanisms of the eye (G. A. Frey), photoreceptor processes (G. Wald), neural activity in the retina (R. Granit), central mechanisms of vision (S. H. Bartley), and a final general chapter on central control of sensory receptors and transmission systems (R. B. Livingston).

Multiple authorship, although essential if accounts are to be authoritative, introduces difficulties in the compilation of any volume such as this. Scientific eminence is not invariably associated with lucidity of exposition; and not all the chapters are equally informative reading for the unsophisticated student of neurophysiology such as I. Each reader will no doubt seek to satisfy his own particular needs and will be variably pleased with the result. As a teacher, I found most complete satisfaction in chapters written, for the most part, by those who are teachers. Investigators in neurophysiology will no doubt bestow their accolades on others. However, the volume as a whole is readable, and differences in style and clarity of expression do not seriously compromise its usefulness.

A second difficulty of multiple authorship is inadvertent omission of topics or cursory treatment with the expectation that others will provide adequate coverage. I shall mention but two examples. In the chapter on transmission of the nerve impulse, the treatment of the Hodgkin-Huxley thesis is compressed into a little more than 1 page out of a total of 45. In the chapter on synaptic transmission, major emphasis is placed on postsynaptic events; little attention is devoted to presynaptic ones. Problems of this nature may be solved by having an introductory or concluding chapter that summarizes, gives perspective to, and supplements the materials presented in the chapters of any one section. Indeed these may have been the instructions given to those chosen to write introductory chapters. In any event, Eccles performed this task in exemplary fashion for the section on neuron physiology.

Each chapter follows a basic plan, yet individuality of expression does not seem to have been stifled by too rigid formalization. A summary of chapter contents is presented first, and this is followed by a brief historical résumé. Because this résumé emphasizes major contributions of the past, only a person thoroughly conversant with both the old and new literature could present it succinctly and in proper perspective. Each author then continues with the development of the present status of his subject, presenting a background of classical concepts, a description of modern investigative methods, quantitative data, and new ideas that have emerged from recent work. Each chapter ends with an extensive bibliography; unfortunately, the titles of original journal articles are not given.

The book is profusely illustrated and the quality of illustrations is good. However, many of the illustrations, because they do not fit into either a single or a double column, are placed off-center and to one side, and the legends are placed off-center and to the opposite side. Space is wasted, and the page imbalance is compounded: page 285 is a horrible example of the way not to arrange illustrations. Just why figures, slightly too wide to fit into a single column, were not further reduced, and why those a bit less than double-column width were not reduced less are mysteries to me. While such liberties with symmetry do not impair my comprehension, they do hurt my esthetic sensibility.

The *Handbook of Neurophysiology* is not a textbook in the usual sense; it is far too detailed for either the casual reader or the beginning student. Furthermore, it presupposes a reasonable background of information and some knowledge of current literature. It will no doubt be of greatest interest and use to teachers of physiology and to beginning investigators. However, in volume 1 the ophthalmologist and otologist will find a wealth of fundamental information on visual, vestibular, and auditory processes. The neurologist and neurosurgeon will find much of interest and value, not only in volume 1, but no doubt in the next two volumes as well. These clinical specialists will profit from a study of volume 1 in direct proportion to their interest in the fundamental aspects of their respective disciplines. Little applied pathophysiology is included in any chapter except that which deals with epileptic seizures. Volume 1 will serve as a reference and bibliography source for many in all fields of biological science. The index prepared by W. Himwich will be particularly helpful to those who use the book in this fashion.

The organization of volume 1 is logical, with one exception. The section on brain potentials and rhythms seems out of natural order; just why it was placed between the sections on neuron physiology and sensory mechanisms is not immediately apparent.

Reviewing the first volume of a three-volume series is somewhat akin to reviewing a three-act play after seeing only the first act. Both are hazardous undertakings, for what may seem an inconsistency or an omission in the first volume or act may be clarified, justified, or included later. The publication of volume 1 of the *Handbook of Neurophysiology* is a significant accomplishment. Coupled with volumes 2 and 3, it will constitute a monumental treatise on the subject.

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Science and Public Policy. Dael Wolfle. University of Nebraska Press, Lincoln, 1959. 81 pp. $1.50.

From the vantage point of his position as executive officer of the American Association for the Advancement of Science, Dael Wolfle is well qualified to speak authoritatively and informatively on the intricate interrelations of science and public affairs. In these three pithy and well organized Montgomery Lectures delivered last spring at the University of Nebraska, he sets forth with commendable clarity and incisiveness the nature of the basic administrative, organizational, and policy problems which confront government, industry, education, and the individual citizen as a consequence of the development of science and technology as dominant factors in contemporary society.

Wolfle's statements of basic principles are sound and sensible and rooted in practical experience. Intelligent formulations and observations abound. This brief volume will undoubtedly be frequently quoted in discussions of the interconnections of science and society. Its succinct review of the scientific revolution and of its impact upon industry, the federal government, and educational institutions provides an excellent summary of recent developments.

Wolfle pleads for an increase in the level of support for basic scientific research by industry and private philanthropy, as well as by government; for recognition of the question of over-all top-level guidance for scientific activities as the number one problem facing science in the federal government; and for deliberately planned innovations in our educational system which will permit significant improvements while retaining the principle of freedom of action by states and local communities. His definitions of fundamental problems and issues are precise and valuable.

He is less persuasive, however, in his proposed solutions. Wolfle presents his own positions logically and eloquently, but one is not always certain that he has included all the relevant considerations in his analyses. It is questionable, for example, whether the "ultimate solution" of the difficulties inherent in the relations between scientific research and the military lies in the re-establishment of an Office of Scientific Research and Development along the lines of the justly praised World War II agency. Wolfle also supports the Berkner proposal for a limited type of Department of Science and Technology and the Klopsteg plan for financing higher education. Whatever the final fate of these suggestions, we can be grateful to Wolfle for his forthright and thoughtful presentation of the issues involved. Participants in future debates of these proposals will have to respect his considered judgments.

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Méthodes Numériques, Interpolation, Dérivées. J. Kuntzmann. Dunod, Paris, 1959, xviii + 253 pp. Illus. F. 3600.

In the recent past, there has been a dearth of books on numerical analysis in French; the only available book was Mineur's *Techniques de calcul numérique*. Now the French have started to catch up, and if the present volume is any indication, the output will be of high caliber. *Méthodes numériques* is a thorough discussion of the field of interpolation and numerical differentiation. Kuntzmann takes us through the first steps of interpolation, deriving all the classical results and introducing some new ones. An important feature is his list of operation counts for the various methods of interpolation. This is obviously very important in deciding on the method to be used, especially in machine computation, where speed outweighs other considerations.

After some further sophisticated discussion of interpolation, the book goes on to numerical differentiation, a field in which the author has made many important contributions. However, in my opinion, the difficulties and dangers involved in numerical differentation are not sufficiently stressed. Other topics, including interpolation and differentiation in the complex plane and in spaces of many dimensions, are then discussed. Finally, a general theory of linear interpolation is developed, and some simple examples of nonlinear interpolation are studied.

The general layout of the book is unsatisfactory. While there is a comprehensive table of contents, there is no index. Furthermore, references are scattered throughout the text instead of being organized in one location. This is unfortunate since there are many useful references to tables and papers which are unavailable elsewhere. An appendix, listing the main formulas useful in practice or at least listing their location in the text, would have been most useful. While these faults limit the book's usefulness as a reference work, they in no way diminish its value as a comprehensive text in this branch of numerical analysis, which is becoming more important in these days of satellite tracking.

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The Rainbow. From myth to mathematics. Carl Boyer. Yoseloff, New York, 1959. 376 pp. Illus. $10.

This is the history of man's view of one physical phenomenon sufficiently spectacular to attract the attention of primitive man and sufficiently complex to attract that of the modern scientist. It would be difficult to imagine a case history better adapted to demonstrate the interest and value of the history of science. Carl Boyer has shown in previous works his competence to deal with both science and the history of science and this book is authoritative and eminently thought-provoking.

He begins with an account of the rainbow in ancient mythology and concludes with the work of Aichi and Tanakadate (1906). "The twentieth century so far has not contributed to the story of the rainbow on so spectacular a scale as did the seventeenth and nineteenth centuries; . . ." (page 320). In telling his story he is, of course, obliged to describe the development of most of the principles of optics, and to a considerable extent he has written a history of that science.

The organization of the book is rigorously chronological. Such organization may be inevitable in a pioneer work on an unfamiliar subject, but it leads to a certain monotony in the recitation of a long succession of opinions, most of which repeat earlier views. Although writers are commended or scolded for their advance toward, or regression from, the modern mathematician's theory of the rainbow, the author draws relatively few general conclusions. The thoroughness and the logical organization of his work make it a source book which will suggest generalizations to others, and it is one of the few comprehensive studies of early physics, outside the field of classical mechanics. The names familiar from studies of the latter field are here, and their views are recounted. But on the rainbow, the views of men such as Ptolemy, Archimedes, Philoponus, Alhazen, Buridan, Oresme, Leonardo, Gilbert, Galileo, and Kepler appear to have been of little immediate importance. Some were not interested; others were conspicuously unenlightened on the subject. Who, then, were the men responsible for advances in rainbow theory? Among those whose contributions were most important were Aristotle, Albertus Magnus, Witelo, Theodoric of Freiberg (whose contribution to rainbow theory, partially derived from experiment, is described as "the greatest contribution of the medieval age to physical science" [page 110]), De Dominis, Descartes, Marci, Newton, and Mariotte. With the exception of Descartes and Newton, none of these have been generally regarded as major figures in the development of physics. This expansion of our field of view in the history of physics is probably the greatest contribution of this book.

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Automatic Teaching: the State of the Art. Eugene Galanter, Ed. Wiley, New York; Chapman and Hall, London, 1959. viii + 198 pp. $3.25.

Although the teaching machine as a piece of hardware dates back at least to the last century, enthusiasm for the possible uses of automatic teaching devices is a relatively new development. This book, an early product of that enthusiasm, is a collection of papers and abstracts, presented at a conference on the automatic teaching of verbal and symbolic skills, held at the University of Pennsylvania in December 1958.

The present-day teaching machine, characterized by an emphasis upon small response units, active responding, immediate reinforcement, and the prevention of errors, was conceived by B. F. Skinner, the Harvard psychologist, and his ideas about operant conditioning have largely dominated attempts to automate teaching. A reading of the book leads to the conclusion that his thinking also dominated the conference.

The book itself can be divided roughly into four sections: papers dealing with certain general problems of automatic teaching, summaries of programs in which teaching machines have been used, reports covering several related attempts at automatic instruction (for example, the scrambled text), and three papers that critically evaluate certain aspects of the over-all approach to the issue.

As do most reports of this sort, the book suffers from a disjointedness, a tendency toward repetitiousness, too much detail on occasion but not enough on others, and the failure to include a general introduction to set the stage and integrate the topics under discussion. I suggest that the prospective reader, unfamiliar with attempts at automatic teaching, first read Skinner's

article which appeared in *Science* [**128**, *909 (1958)*]

Much can be said on the positive side. The papers taken together undoubtedly reflect quite accurately the present state of the "art." Furthermore, they contain a great deal of information and many stimulating ideas. The book's most important contribution, however, is in the reiteration of the vast number of questions that must be answered before such techniques can even approximate their maximal usefulness, and the emphasis on the role teaching machines can play as a research tool in providing the answers.

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Rehabilitation of the Mentally Ill. Social and economic aspects. A symposium of the American Psychiatric Association. Cosponsored by the Section on Social and Economic Sciences of the American Association for the Advancement of Science and by the American Sociological Society, December 1957. *AAAS Publ. No. 58.* Milton Greenblatt and Benjamin Simon, Eds. American Association for the Advancement of Science, Washington, D.C., 1959. 260 pp. Illus. Prepaid to members, $4.; others, $4.50.

In general one may say that no symposium should be published as a symposium, for the editorial task of fitting the pieces together and of eliminating redundant repetitions always takes so much time and raises so much resentment that no editor, not even the most conscientious one, is persistent or heartless enough to accomplish the task. Consequently, every published symposium is longer than it should be, and too much fine material is lost in a welter of words. This volume is no exception; if cut to one-third its size, it would be about 10 times as useful. Nevertheless, that which remains is of sufficient value to make the volume timely and worth a good deal of angry digging.

After a foreword and a preface, *Rehabilitation of the Mentally Ill* is divided into four sections: "General problems" (Ewalt, Greenblatt, and Hunt), "Hospital phases of rehabilitation" (Martin, Notman, Landy, Raulet, Key, Wittkower, and Azima), "The transition from hospital to community" (Temple Burling, George Brooks, Knudson, Sanders, and Carmichael), and "The community aspects of rehabilitation" (Ernest Gruenberg, Huxley, Elsa Kris, Bertram J. Black, and Simon Olshansky).

I kept wishing that the simple diagrammatic staircase presented on page *18* had been used as the skeleton of the book. Each aspect of the social rehabilitation of the mentally ill is placed on a step in this staircase. At the bottom is "Hospital," then as one ascends the staircase he goes through "Night hospital," "Day hospital," "Sheltered workshop," "Half-way house," "Family care," "Ex-patient clubs," "After-care clinics," and "The community." Each of these steps could have been a chapter, and all the participants' discussion, properly winnowed, could have been assembled under one or another of these categories. In this way, the whole volume could have been condensed, and the material concentrated instead of scattered.

One topic that is being considered at Fountain House is not discussed here: the preparation of families for the return of patients to the family group. Family groups would be brought together to discuss their problems and their feelings while the patients are still in the hospital but are nearing the time when they will make their first visits home. These could be group therapy sessions in anticipation of the return of patients. (This could also be done with foremen in industry, with employers, and others.)

I found certain chapters exemplary, but it would not be seemly to make invidious distinctions. Furthermore, others will undoubtedly find that other chapters are best for their needs. Therefore I limit myself to one recommendation: those working in this field should take the time and make the effort to extract from the book its essential data, even though they will surely groan over the inhumanity of the symposium format for the ultimate reader.

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New Books

Annual Reports on the Progress of Chemistry, 1958. vol. 55. R. S. Cahn, Ed. Chemical Society, London, 1959.

Approach to Archaeology. Stuart Piggott. Harvard Univ. Press, Cambridge, Mass., 1959. 144 pp. $3.

Chronic Illness in a Rural Area. The Hunterdon study. Reported by Ray E. Trussell and Jack Elinson. Published for the Commonwealth Fund by Harvard Univ. Press, Cambridge, Mass., 1959. 454 pp. $7.50. This volume completes a series of four reports on the problems of chronic illness in the United States. The reports, published under the sponsorship of the Commission on Chronic Illness, are available from the Harvard University Press.

La Culture des Tissus Végétaux. Techniques et réalisations. R. J. Gautheret. Masson, Paris, 1959. 881 pp. F. 10,500.

Dictionary of Discoveries. I. A. Langnas. Philosophical Library, New York, 1959. 206 pp. $5. Alphabetically arranged biographical entries cover explorers, some instrument makers, and some people who financed the journeys. The entries vary from a few lines to a full page or more.

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Reports

Velocity of Light and Measurement of Interplanetary Distances

Abstract. The combined availability of atomic clocks and of instrumented planetoids traveling in their own solar orbits will offer the possibility of determining their distance from us, and hence interplanetary distances, in terms of the wavelength of the radiation of atomic frequency standards. It can be anticipated that the accuracy of these measurements will be very high and will not depend upon our less accurate knowledge of the velocity of light in terms of the standard meter, the sidereal second, and so on.

Ever since observation of the eclipses of Jupiter's moons indicated that the velocity of light is finite—and gave the first measure of that velocity—much effort has been devoted to increasing the accuracy of our knowledge of this important physical constant. The purpose of this report is to suggest that a change of emphasis may be in the offing: As we learn how to make interferometric measurements of distances between earth-borne stations and instrumented planetoids gravitating in their own solar orbits, we may be led to accept c as just c, and to express important parameters of our solar system, such as interplanetary distances, and the products of the masses of the sun and planets by the gravitational constant, in terms of the wavelength and period of the radiation of atomic frequency standards.

Three great classical experiments, and the inventions which made them possible, stand out among the many contributions made toward the measurements of length and of time.

The first is the determination Foucault made of the speed of light, by means of a rotating mirror and over a measured distance, in terms of the standard meter and the sidereal second.

The second is the measurement Michelson made, by means of his interferometer, of the wavelength of the red cadmium line, in terms of the standard meter. Today this line is replaced by the green line of Hg^{198}. It may be noted that these two experiments in combination yield the period of the Hg^{198} green line in terms of the sidereal second.

The third experiment is the measurement made, by means of the atomic clock suggested by Rabi, of the period of certain microwave lines, such as the cesium line, in terms of the sidereal second.

It may be asked whether application of careful optical interferometric metrology to the measurement of the dimensions of cavities or wave guides could not serve to determine the wavelength of the cesium line in terms of the green line of Hg^{198}, but such an experiment would be subject to severe limitations. First, the width of the resonance line of the best microwave cavities, even superconductive cavities, sets an upper limit to the accuracy with which their resonance frequencies may be determined. Second, there is uncertainty about the surface penetration of these microwaves. Third, the fairly large natural relative width of the sharpest optical lines sets another upper limit to the accuracy with which the dimensions of cavities or wave guides may be determined in terms of standard optical wavelengths. These limitations would make any measurements based on cavities or wave guides inaccurate by comparison with the accuracy of our determinations for the period of the cesium line in terms of the sidereal second, or with the yet greater accuracy of agreement between cesium clocks that will eventually be obtained (the accuracy of agreement, at this writing, is already of the order of 1 part in 10^{10}). These new orders of accuracy may well spark the question: What measurements of length will be made eventually with an accuracy depending upon the accuracy of the cesium clock alone, and not upon other limitations such as the low Q's of cavities or the large relative widths of optical lines?

The answer to this question appears to be, the measurement of long distances over line of sight and *in vacuo* by means of radio interferometry, these distances to be measured at first between an earth station and an instrumented planetoid, and the data thus obtained to be utilized subsequently to determine accurately interplanetary distances.

The radio interferometric procedure may be sketched as follows (*1*). Point A emits a continuous-wave signal of frequency f, regulated by an atomic clock. This continuous-wave signal is received at B, where a second signal is generated which is coherent with the first (say a continuous-wave signal of frequency $7f/6$) and which can be emitted by B without interfering with B's reception of f. This second signal is received at A, where it is retransformed into a signal of frequency f. The phase of this second f-signal is compared to that of the original emitted f-signal, and the number of 360° phase changes observed between these two signals during a given time interval is exactly equal to the number of wavelengths by which the AB round-trip distance has increased (or decreased) during that given time interval.

It may be noted that the precision with which the relative phase of the two f-signals can be determined, while subject to certain instrumental limitations, is fairly independent of range. Thus, the fractional accuracy with which distance increments can be measured will increase with increasing distance, and will be determined eventually by $\triangle f/f$, when the distances measured are of the order of magnitude $\lambda f/\triangle f$, where $\triangle f$ designates the uncertainty in our knowledge of f. If we make $\lambda = 10$ cm and assume $f/\triangle f = 10^{10}$, we get 10^6 km for the order of magnitude of the shortest vacuum distances measurable with an accuracy matching the cesium-clock accuracy just assumed. Vacuum distances of the order of 10^6 km are not available on the earth, and the only approximation of such experimental conditions is offered by the measurement of the distance between a point on the earth's surface and an instrumented satellite of long range or an instrumented planetoid. The concomitant requirement that line of sight be maintained at all times between the measuring points rules out most satellites but allows the measurement of distance between an earth station on a firm polar cap (Antarctica) and an instrumented planetoid gravitating on a suitable solar orbit.

The suggestion of a planetoid instrumented for radio interferometry is attractive for additional reasons. First, there is an uncertainty of several wavelengths (at S-band) in the distance

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

measurement, due to imperfect knowledge of the refractive index in the small portion of the range included in the atmosphere, and due also to the unknown deformation of the earth crust. When the measurements are made over interplanetary distances, these uncertainties become much smaller than 1 part in 10^{10}. Second, the ranges involved, even though enormous when compared to anything with which we have any experience (three orders of magnitude above "moon-radar" ranges), are quite manageable with modern radio techniques. Indeed, it has been calculated that isotropic planetoid emissions of the order of 1 watt will yield more than adequate signal-to-noise ratios at the earth station. And third, comparison of the orbital data thus obtained with the orbital data of the planets would increase the accuracy of our determinations of interplanetary distances by several orders of magnitude.

This last point is the crux of the matter. Interplanetary distances are known today with an absolute accuracy of the order of 1 part in 10^8 only, owing to the poor degree of accuracy with which we know the parallax of the sun. On the other hand, interplanetary distances relative to each other are known with a much higher degree of accuracy, of the order of 1 part in 10^9. Thus, the availability, for a single orbit, of data known with an accuracy of the order of 1 part in 10^{10} would yield immediate knowledge of the other interplanetary distances with an accuracy of 1 part in 10^9. This means that a single successful planetoid experiment would increase the absolute accuracy with which interplanetary distances are known from a figure well below that realizable in laboratory measurements to a figure well above it. This means also that later and more elaborate planetoidal experiments will permit closer checks of the relativisitic gravitational correction, more accurate estimates of the distribution of large masses and of dust within the solar system, and so on. It even appears permissible to begin to speculate about the possibility of detecting spatial curvature or departures from the gravitational theory by means of these techniques.

It is also worth noting that if the experiments forecast here are successful, the standard meter, the wavelength of the green line of Hg^{198} and the sidereal second will play a secondary part in these measurements. The periods of the satellites and planets will be measured eventually in terms of (for instance) the cesium line period t_o; the orbital dimensions will be measured in terms of the wavelength of the cesium line λ_o; the product of the mass of a planet and the gravitation constant will be measured in terms of $\lambda_o^3 t_o^{-2}$, and so on. We shall have of course $\lambda_o = ct_o$, by definition, but any determination of c in terms of "secondary" standards, such as the standard meter, the sidereal second, or the wavelength of the green line of Hg^{198}, will be of much lower accuracy than the measurement of planetary parameters for which λ_o and t_o will be the newly adopted primary standards.

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Note

1. A detailed and quantitative description of this technique has been published (M. J. E. Golay, "Interferometric rocket guidance," *Conf. Proc. PGMil of IRE, 2nd Natl. Conv.*, p. 182).

6 July 1959

High-Resolution Density Gradient Sedimentation Analysis

Abstract. The principle of stability for a sample layered in a density-gradient liquid column is discussed, and a method for separating ribonucleoprotein particles by means of sedimentation in the ultracentrifuge is described.

In the process of our studies of bacterial ribonucleoprotein particles (ribosomes), the need arose for a method of sedimentation analysis which would supply separated samples of the ribosomes of *Escherichia coli* (*1–3*). These particles have sedimentation constants of about 20, 30, 50, 70, and 100*S*. Adequate separation of these classes has been obtained by sedimentation at 105,000*g* through a density-gradient stabilized liquid column, use being made of the principle that the stabilizing density gradient must always exceed the inverted density gradient introduced by the sample. Since the method is mechanically simple and probably of general application, it is described separately in this report.

Density gradients are commonly applied to prevent mixing in liquid columns which are used for zone analysis by means of centrifugation or electrophoresis. However, this method has been limited to very small quantities of material, since the sample itself may introduce a region of density instability. So long as the density increases in the direction of the gravitational (or centrifugal) field, the gradient will exercise a stabilizing force against mixing which occurs as a result of mechanical disturbances or temperature gradients. If the density gradient is locally inverted due to the presence of a sample, the liquid containing the sample will stream through the less dense underlying layers. This process does not necessarily stop when the stream reaches a region of equal density, since the stabilizing solute (usually more rapidly diffusing than the sample) will diffuse into the stream and may continually reestablish a condition of instability.

The sample layer shown in Fig. 1*A* is initially stable. However, as soon as the sample is moved downward (or the stabilizing solute diffuses into the sample layer), the inverted density gradient will cause streaming. The inverted *density* gradient may be avoided if the sample is introduced (Fig. 1*B*) with a *concentration* gradient opposite to that of the stabilizing gradient, provided the inverted density gradient due to the sample itself is significantly less than the stabilizing density gradient.

Usually a maximum quantity of sample can be analyzed when both gradients are linear. The amount of sample which can be handled rises with the square of the width of the sample layer, since it is the *gradient* in density and not the maximum density of the sample which determines stability.

The instability of sharply defined sample layers has been previously recognized and offset (*4*) by stirring the sample layer to reduce the inverted gradient at the lower edge of the band. Stable inverted sample gradients of complex shape have been created through the use of mixing chambers (*5*). However, the large sample capacity and the simplicity of the inverted linear sample gradient have not previously been mentioned in the literature.

This principle has been applied successfully both for analysis by electrophoresis and for analysis by sedimentation in the ultracentrifuge. Since the latter application has been of great importance to our experiments on synthesis of (*1, 2*) and by (*6*) the ribosomes of *E. coli*, it is described here in detail.

A linear stabilizing density gradient of sucrose (20- to 5-percent) was prepared with a modification of the linear gradient mixing device of Bock and Ling (*7*), shown in Fig. 2. Such gradients are stable for many hours, and only moderate care need be taken in handling the tubes. The same device was then used to introduce the inverted sample gradient. For this purpose the left-hand chamber was loaded with 0.2 ml (for example) of buffer containing 5 mg of ribosomes per milliliter, and the right-hand chamber with 4-percent sucrose in the same buffer. The sudden step in sucrose concentration from 5 to 4 percent makes it possible to start the sample gradient without undue mixing. The stabilizing gradient can be reduced when the sample to be analyzed is small. Gradients of 10- to 3-percent sucrose

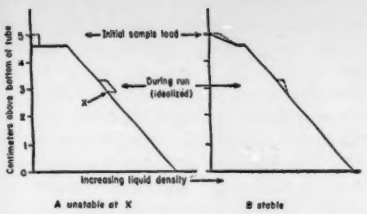

Fig. 1. Conditions for stability in a density gradient liquid column. Solid line, stabilizing gradient; dotted line, contribution due to a sample in process of analysis.

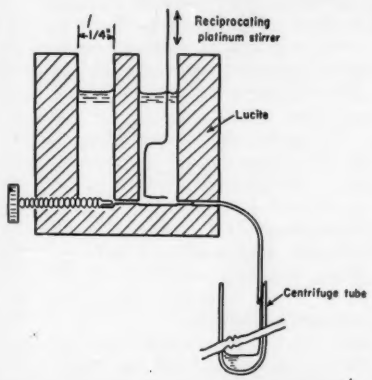

Fig. 2. Mixing chamber for the production of linear stabilizing gradients. The left chamber is filled with 2.4 ml of 5 percent sucrose, and the right, with 2.2 ml of 20-percent sucrose. After filling and after starting of the mixing motor, the center valve is opened and the exit tubing is turned down to touch the side of the centrifuge tube. The sucrose solution then runs down the wall of the tube. If the 4.6 ml are delivered in 10 to 15 minutes, the succeeding lighter solution floats on the underlying liquid with little mixing.

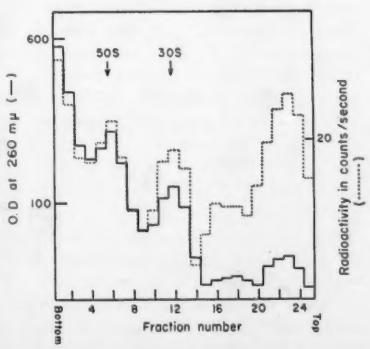

Fig. 3. Example of the use of density gradient stabilized sedimentation analysis for measurement of radioactivity incorporated into the smaller ribosomes during 1-minute exposure to $S^{35}O_4^{--}$. Solid line, optical density at 260 mμ of 0.2-ml samples diluted to 1.2 ml. Dotted line, trichloroacetic acid precipitable radioactivity.

have been used occasionally and are quite adequate.

This whole process was carried out in the cold room with solutions at 1° to 4°C. The tube was loaded into the precooled swinging bucket rotor (Spinco SW39) and centrifuged for the appropriate time. As quickly as possible at the end of the run, the tube was gently lifted out of the rotor and mounted in a device which perforated the bottom of the centrifuge tube with a hypodermic needle (ground to a short, double-sided point) located about 1 mm above the bottom of the tube. After removal of a piano wire which kept the hypodermic tubing clear and free of air bubbles, the contents of the tube were run out in 25 equal cuts by drop counting.

Figure 3 shows the result of use of this system for examination of the radioactivities of the smaller particles of *Escherichia coli* after a short period of incorporation of radioactive sulfate. A growing culture of *E. coli* was starved of sulfur for 30 minutes, and then $S^{35}O_4^{--}$ was added. After 1 minute, carrier $S^{32}O_4^{--}$, S^{35}-cystine, and S^{35}-methionine were added to displace the radioactivity of the rapidly-turning-over soluble proteins synthesized by the ribosomes (*6*). Fifteen seconds later the culture was suddenly chilled to 0°C, the cells were washed and broken, and the ribosomes were harvested in the preparative ultracentrifuge. The ribosome pellet was resuspended in appropriate buffer, and a sample was loaded in an inverted linear gradient layer, as described above. The tube was then centrifuged (37,000 rev/min, for 150 minutes) to pellet most of the 70 and 100*S* ribosomes in order to spread out the 20-to-50*S* region for a close examination of the smaller ribosomes.

The peaks in the diagram correspond closely with the peaks shown by the analytical centrifuge which was used to determine the sedimentation constants.

A series of experiments of this type (*2*) shows that the newly synthesized ribosomal protein and ribonucleic acid appear first in the smaller ribosomes (20, 30, 50*S*) and later in the larger ones (70, 100*S*).

A sample of the width discussed above has provided the best compromise between high resolution and a useful quantity for analysis, although the resolution could probably be improved to the limit set by diffusion if smaller samples and great care were used. For the example shown in Fig. 3, the full width (½ max.) due to diffusion alone would be about 1 mm or about ⅛ of the observed width. Since the width is due to causes other than diffusion, equivalent resolution could be obtained for objects very much smaller than the ribosomes. For example, serum globulin (molecular weight, 170,000, about 7*S*) would require about 12 hours of centrifugation, and the resulting diffusion width would be about 3 mm. For the preparation of large quantities of material, very broad sample gradients are useful. In the preparation of pure 30*S* particles from a mixture of 30 and 50*S* ribosomes, 2.4 ml of a very concentrated ribosome suspension are placed in the left chamber, and 2.4 ml of 20-percent sucrose, in the right chamber. In this way 10 to 20 mg of pure 30*S* particles may be obtained in a single run.

It is worth noting that, while the centrifugal force doubles from the top to the bottom of the tube in the swinging bucket rotor, the viscosity of 20-percent sucrose is about twice that of water. When correction is made for the density of the sucrose solution, it is found that the sedimentation velocity of ribosomes is very nearly constant throughout the length of the centrifuge tube.

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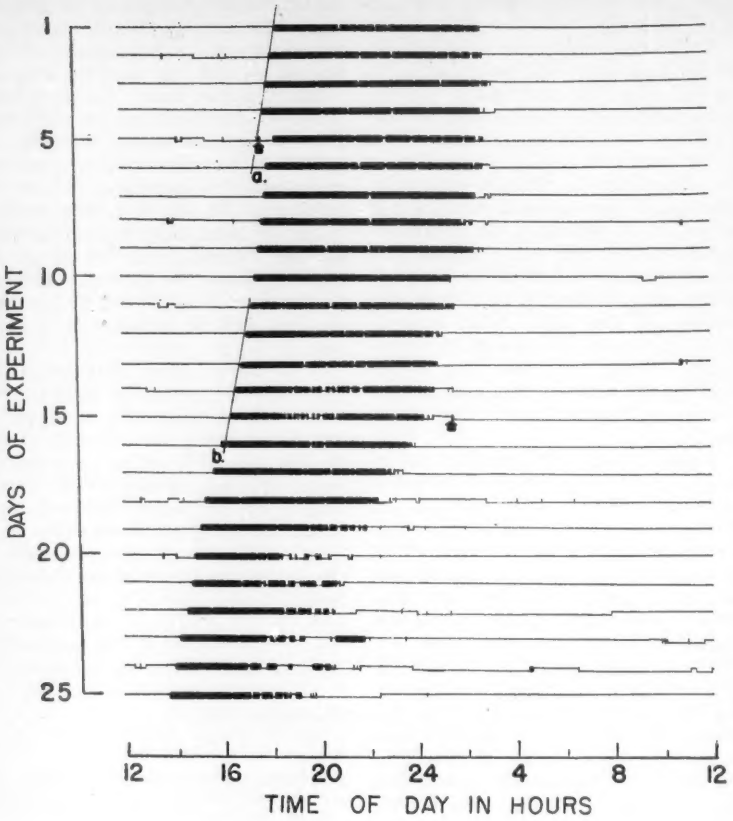
31 July 1959

Daily Light Sensitivity Rhythm in a Rodent

Abstract. Single 10-minute light periods can cause a phase shift in the rhythm of the daily locomotor activity of flying squirrels otherwise maintained in constant darkness. A daily rhythm of sensitivity to these standard light periods was found.

Recent investigations have demonstrated that many animals in isolation conditions exhibit a precise rhythm of locomotor activity which appears to be controlled by a "biological clock," characteristic in cycle length for each individual animal. Nocturnal rodents show cycles ranging from about 22 hours to about 25 hours in length for different individuals (*1*, *2*). These endogenous rhythms may be synchronized by clues



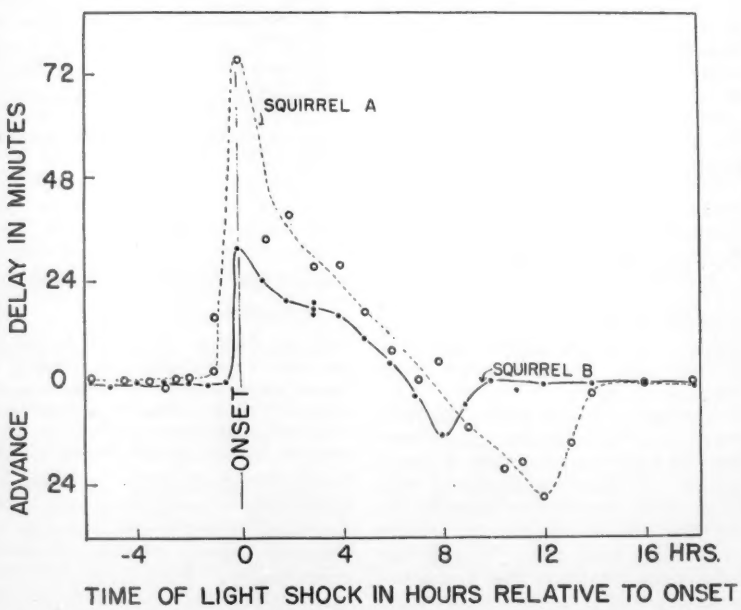


Fig. 1 (top). Activity record of a flying squirrel, showing light-shock effects: (a) delay shift in phase caused by a light shock starting 1 minute after the beginning of the daily activity period; (b) advancing phase shift caused a light shock 9 hours after the onset of activity. Arrow indicates light periods. Fig. 2 (bottom). Light-sensitivity curves for two flying squirrels, A and B.

in the environment, such as the light-dark cycle of a natural or artifical day (*1*).

It has been suggested that such synchronization is brought about by a rhythm of sensitivity of animals to light. In *Peromyscus*, feeding disturbances with a dim light during the sensitive phase caused a delay in the start of activity the following night, while a similar disturbance in the insensitive phase had no effect on the activity rhythm (*3*). With standard light exposures of several hours' duration, rhythms of light sensitivity have been shown in the hamster (*4*), and in *Euglena* (*5*). Cultures of *Drosophila*, when exposed to light, showed phase shifts dependent upon the time in their daily hatching rhythm at which light occurred (*6*).

To further describe the effect of light on the endogenous rhythm, a quantitative determination was made of the rhythm of light sensitivity; short-term "light shocks" of standard duration and intensity were used at hourly intervals during the daily activity cycle. For this study flying squirrels, *Glaucomys volans*, were housed individually in recording wheel cages. A nest chamber, supplied with excess food and water, provided free access to a running wheel mounted on a bicycle axle. Revolutions of each squirrel were indicated on an Esterline-Angus Operations Recorder. Cages were maintained individually in light-tight cabinets, in constant darkness, at a uniform temperature ± 1°F per day.

The 24-hour records for each individual were mounted in a consecutive vertical series and showed that a squirrel alternated approximately 12 hours of inactivity with 12 hours of intense running in the wheel (Fig. 1). By measuring the time between successive daily onsets of running in the wheel in constant darkness (*2*), it was possible to determine the slope of the onset of activity and thus to predict the start of activity on a particular day. At intervals of 4 to 25 days the squirrels were exposed to 10 minutes of light of an intensity of 0.5 ft-ca. A phase shift was calculated as the difference between the expected and the actual time of activity onset for the activity period following the light shock (method shown diagrammatically in Fig. 1).

These phase shifts, illustrated graphically in Fig. 2, demonstrate a marked rhythm of light sensitivity. The form of the curve followed a similar pattern for 12 squirrels tested. Maximal delay effect occurred with light at the onset of running in the wheel. A 6- to 7-hour period of gradually decreasing delay phase shifts followed. At about 7 hours after the start of running a change was made

to a short period of resetting by advancing the time of onset, until at 10 to 14 hours after the onset of activity the squirrel reached the inactive, light insensitive part of its cycle. No phase shifts were caused by light during the remainder of the inactive period except during the last hour before the start of running in the wheel.

While sensitivity curves for different squirrels showed a similar form, they differed slightly from each other in details. Striking differences in the amount of phase shifting caused by equivalent light shocks were common for different individuals. Likewise, small differences in the time relationships of the two curves are apparent in Fig. 2.

Such a rhythm of sensitivity to light has been found adequate to explain the stepwise synchronization of the active period of all flying squirrels tested, regardless of their cycle lengths, to the time of darkness in artificial or natural days (*7*). A daily rhythm of light sensitivity also serves as a basis for the interpretation of many previous studies of the effect of light upon the activity cycles of rodents (*8*).

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24 August 1959

Heat-Labile Serum Systems in Fresh-Water Fish

Abstract. Serum specimens from 18 specimens of 12 different species of fresh-water fish were examined for their ability to kill *Toxoplasma* nonspecifically. This ability was present in all sera except those of two of three great northern pike. The effect was destroyed by exposure to 53°C, 56°C, or zymosan. Complement was demonstrated in all sera except that from one great northern pike, when rabbit erythrocytes were used in the indicator system.

Specific toxoplasma antibody, when measured in the dye test (*1*), requires the presence of a heat-labile serum system (activator), similar to, if not identical with, the properdin system (*2*, *3*). "Activator" can be measured quite accurately in human serum, but its detection in other species is difficult because, generally, they possess a heat-labile, nonantibody dependent factor(s) which kills *Toxoplasma*, giving them an unstained appearance in the dye test. Aside from the mouse (which has neither the "activator" nor the antiparasite effect), the heat-labile, nonspecific system has been demonstrated in the sera of rats, monkeys, cattle, sheep, horses, swine, mink, rabbits, guinea pigs, dogs, cats, pigeons, chickens, and ducks. With the exceptions noted, it thus appears to be a characteristic of most, if not all, species of mammals and fowl. Recently, an opportunity presented itself to extend these observations to several varieties of fresh-water fish. This report includes data on both nonspecific antitoxoplasmic factor and hemolytic complement in the sera of 12 species of fish.

The fish were obtained from Oneida Lake, New York, by netting (*4*). Their ages were undetermined except that all appeared to be mature specimens. Bleeding was accomplished by cardiac puncture (without anesthesia), using sterile needles and syringes. The bloods were transferred to sterile test tubes and chilled in wet ice until returned to the laboratory 1 to 2 hours later, where the sera were separated from the clots and promptly frozen and stored in dry ice. The specimens were thawed rapidly just prior to use and kept cold in an ice-water bath throughout the dilution procedure.

Testing for the nonspecific factor was accomplished by mixing 0.2 ml of either the undiluted or serial twofold dilutions of the serum with 0.05 ml of fresh mouse peritoneal exudate containing large numbers of *Toxoplasma* (RH strain); sufficient heparin was added to the exudate to make a final concentration of 1:10,000 in each tube. The tubes were incubated for 1 hour in a 37°C water bath, alkaline methylene blue was added, and the stained and unstained parasites were enumerated in the usual fashion (*1*). All serum dilutions were made with 0.85-percent salt solution. The titer of the nonspecific factor was taken to be the reciprocal of the original dilution of serum in which 50 percent of the parasites were unstained. A control tube was included in which the undiluted serum had been heated at 56°C for 30 minutes (*5*). In addition, some sera were examined after heating at 53°C (*6*) for 25 minutes; the results were almost identical. One specimen of carp serum was treated with zymosan at 17°C (*3*) and then tested for the nonspecific factor. In one experiment, the coagulation of aliquots of catfish, carp, and smallmouthed bass bloods was prevented by heparin and Versene, but these sera reacted no differently from that obtained from spontaneously coagulated blood. Hemolytic complement was assayed in the fish sera by testing with sheep erythrocytes in some instances and in others with rabbit red cells according to the recommendations of Cushing (*6*). The various data obtained from 18 of the fish (12 species) are summarized in Table 1.

It will be noted that, to varying degrees, all of the sera contained nonspecific, antitoxoplasmic factor except the sera of two of the three great northern pike. The sera of the third pike was minimally positive in a dilution of 1:2. One of the inactive sera was obtained from a mature specimen, 40 in. long and about 14 lb in weight. It is interesting that the five pike perch, a species closely related to the great northern pike, were all positive and to the same marked degree, 1:32.

In each instance, the antitoxoplasmic effect was eliminated by heating for 30 and 25 minutes at 56° or 53°C, respectively, although in a few of the latter cases, questionable activity remained in the undiluted serum. The one carp sample that was treated with zymosan

Table 1. Nonspecific heat-labile antitoxoplasmic and hemolytic complement titers of 12 species of fresh-water fish. Heat of 56°C eliminated the heat-labile, nonspecific, antitoxoplasmic factor. The effect of 53°C heat on the complement is shown below.

Titer nonspecific factor	Complement (rabbit red blood cells)	Effect of 53°C heat on complement
Bullhead (*Ameiurus nebulosus*)		
1:2	Undiluted	±
Carp (*Cyprinus carpio*)		
1:8*	1:8†	±
Catfish (*Ictalurus punctatus*)		
1:4	1:4	Eliminated
Common sucker (*Catostomus commersonii*)		
1:2	1:4	Eliminated
Dogfish (fresh-water) (*Amia calva*)		
1:4	1:4	Eliminated
Eel (*Anguilla chrysypa*)		
1:2	1:8	Eliminated
Great northern pike (*Esox lucius*)		
None found	None found	Not done
None found	1:2	Not done
1:2	1:8	+
Largemouthed bass (*Huro salmoides*)		
1:4	1:8	±
Ling (*Lota maculosa*)		
1:2	1:2	Eliminated
Pike perch (*Stizostedion vitreum*)		
1:32	1:8‡	Undiluted
1:32	Not done	Not done
1:32	Not done	Not done
1:32	Not done	Not done
1:32	Not done	Not done
Smallmouthed bass (*Micropterus dolomieu*)		
1:64	1:16	Eliminated
Yellow perch (*Perca flavescens*)		
1:16	Not done	Not done

* Eliminated by treatment with zymosan at 17°C.
† None demonstrated with sheep red blood cells.
‡ 5.5 μ/ml with sheep red blood cells.

became negative for the nonspecific factor. In previous, unpublished studies with rabbit sera, we found that treatment with zymosan uniformly eliminates the nonspecific factor.

When complement determinations were attempted with a sheep red blood cell system, no activity was demonstrated in one carp, but one pike perch had 5.5 units/ml. These were the only two specimens examined in this manner. The remaining samples were titrated for complement with rabbit red cells (*6*) and all were positive except one of the great northern pike. Both the nonspecific and complement activities (as determined with rabbit cells) were either present or absent from the same sera, except in one northern pike.

Except for previous studies of complement in carp by Cushing (*6*), little is known of the heat-labile serum factor content of fish sera. Such sera may be readily obtained with very little practice by cardiac puncture and the fish may be kept alive for several bleedings. Some 90 ml of blood were procured in separate bleedings on two consecutive days without killing a carp. The eel presented something of a problem because it was difficult to hold, and oozed a considerable amount of oil. We believe, however, that blood, rather than a mixture of blood and oil, was obtained.

It seems reasonable to assume that the heat-labile, antitoxoplasmic system in the fresh-water fish is similar to, if not identical with, that of mammals and birds. Although toxoplasma infections have not been demonstrated in fish, the presence of the heat-labile serum activities is probably a fortuitous biologic coincidence, for it would seem unlikely that they were acquired as a specific response to any infectious agent. Properdin, complement, and Mg^{++} (*2*) make up a portion of the nonspecific system which probably also possesses other as yet unknown components, as well.

The behavior of the great northern pike is of special interest, since two of the three specimens were inactive. In fact, after the first was found to be negative, the other two specimens were acquired to check this initial experience. Since the great northern pike, pike perch, and muskellunge are closely related, it would be of considerable interest to examine the latter. This is to be attempted as specimens become available. Whether the sera of salt-water fish possess heat-labile activities comparable to those of the fresh-water fish also remains to be determined (*7*).

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5. Human "activator" was added to a second control tube of similarly inactivated undiluted serum; the dye reactions were uniformly negative, indicating that the fish sera did not contain specific antitoxoplasma antibodies.
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Chemically Induced Phenocopy of a Tomato Mutant

Abstract. Lanceolate, a spontaneous leaf-shape mutant which fails to produce cotyledons and plumule in the homozygous condition, shows development if supplied with either adenine or a diffusate obtained from normal seeds. Similar development occurs in a different genetic background.

The leaf of the normal tomato, *Lycopersicon esculentum*, is odd-pinnately compound. In contrast, a leaf-shape mutant found by Casseres of Costa Rica has a simple, entire, elongated leaf which we called lanceolate. Associated with the lanceolate leaf shape are small fused cotyledons, early appearance of axillary shoots, which indicate weak apical dominance, leafy inflorescence, and reduced flower size.

On selfing, lanceolate always segregated normal and lanceolate plants approximately in a 1:2 ratio. In addition, about a quarter of the seeds either failed to germinate or appeared as narrow or reduced phenotypes (Table 1). Reduced seedlings never developed beyond a cylindrical mass of green tissue about 5 cm tall and 0.2 cm in diameter, without a trace of cotyledons or plumule. When ungerminated seeds from lanceolate plants were dissected, about one quarter of the embryos were completely devoid of cotyledons and plumule. We assume that such embryos would give rise to reduced seedlings under favorable conditions of germination. Narrow plants, on the other hand, had the appearance of an extreme lanceolate. In contrast to reduced seedlings they had cotyledons and plumule; furthermore, they produced shoots, leaves, and aborted inflorescences, but never flowers. If narrow and reduced phenotypes and the ungerminated seeds are considered to be homozygous lanceolate, the segregation in the upper part of Table 1, which shows the segregation from selfed lanceolate and from the hybrid lanceolate × lanceolate, is not significantly different from a 1:2:1 ratio. That lanceolate is a heterozygote is also clear from the progeny of lanceolate crossed with normal: both lanceolate and normal plants appear in about equal proportions (lower part of Table 1). In effect, the normal allele behaves as a recessive. In accordance with long-established usage, lanceolate is considered to be a dominant allele because it produces an observably different phenotype in the heterozygous condition, and regardless of the fact that homozygous lanceolate may be expressed as an even more extreme phenotype (narrow or reduced).

It is evident that the lanceolate gene has an effect on the growth of cotyledons and leaves. One lanceolate allele reduces the size markedly; with two there is even greater reduction in leaf size (as in narrow) or complete absence (as in reduced). Thus, the lanceolate gene may be responsible for a deficiency of some growth substance. Several authors have identified substances with growth-promoting properties that may have some relevance. Went (*1*) found that there was in the cotyledons of peas a substance that promoted leaf growth. It could diffuse out of germinating seeds and stimulate growth of excised pea leaves in culture medium (*2*). Bonner and Haagen-Smit (*3*) have shown that adenine influences the growth of mesophyll but has no influence on the growth of veins. On the other hand, Went and Thimann (*4*) have shown that auxin increases the growth of veins, but not the growth of mesophyll. Finally, Miller and Skoog (*5*) concluded that the initiation and development of buds from tobacco pith cells in vitro was dependent

Table 1. Segregation in selfed and hybrid cultures of lanceolate.

	Normal	Lanceolate	Narrow	Reduced	No germ.	Total
Lanceolate selfed						
Germinated in soil	22	58	1	0	24	105
Germinated on filter paper	35	68	0	37	0	140
(F_1 lanceolate from lanceolate × broad) × lanceolate	10	33	11	0	0	54
Total	67	159		73		299
Exp. (1:2:1)	75	149		75		299
Lanceolate × normal	19	15				34
Exp. (1:1)	17	17				34

Table 2. Effects of diffusate and adenine on germinating seeds of lanceolate.

Normal	Lanceolate	Reduced	Modified	Total
		Diffusate		
15	25	4	6	50
		Adenine		
	39	8	3	50

Table 3. Effect of various adjuvants on reduced embryos grown in White's medium.

Treatment	Reduced	Modified	Total
(i) Strong diffusate	4	1	5
(ii) Weak diffusate	1	3	4
(iii) Adenine	2	3	5
(iv) Adenine + indoleacetic acid	2	3	5
(v) Indoleacetic acid	5	0	5
(vi) Distilled water (control)	5	0	5

upon the proper balance of adenine and indoleacetic acid (auxin).

Our first success in attempting to obtain growth in the reduced phenotype came by treating germinating seeds of lanceolate with diffusate and adenine; the seeds, of course, contained embryos in the proportion of 1 normal : 2 lanceolate : 1 reduced. Diffusate was prepared in the following manner: 500 normal tomato seeds were soaked in 20 ml of distilled water. After 3 days the water, with diffusible substances, was removed, and distilled water was again added. This procedure was repeated daily for a total of 4 days. The solution removed from the seeds was lyophilized to dryness, and the powder was dissolved in 10 ml of distilled water. Fifty seeds of lanceolate were germinated on filter paper in a Petri dish which contained the 10 ml of concentrated diffusate. Of the 50 resulting seedlings, four were typical reduced plants but six were a new phenotype, which we called "modified" (Table 2). Instead of remaining as a cylindrical mass of tissue, the hypocotyl broadened at the tip to form a structure resembling a cotyledon, at the base of which a bud developed that later grew into a shoot. The mature plant that developed from this shoot was very similar to the narrow phenotype, although the latter always had two cotyledons and a plumule to begin with. A second 50 seeds were treated with 40 parts per million of adenine sulfate; they produced eight reduced seedlings and three of the modified phenotype.

Tomato seeds usually complete their germination in 7 to 10 days; however, it was noted that the normal seeds from which the diffusate had been removed failed to complete germination. The roots developed almost at the normal rate, the hypocotyl elongated only slightly, but the cotyledons never emerged from the seed coat, nor did they show any sign of growth. Thus, a substance or substances required for early cotyledon and shoot growth may diffuse out of the germinating seeds. If the substance or substances are continuously removed from the seeds, normal growth will not take place.

In a second experiment seeds from selfed lanceolate plants were soaked in distilled water for 3 days, then dissected under sterile conditions. Embryos without cotyledons, presumably homozygous lanceolate, were selected. These were placed individually in test tubes on an agar surface. All of the test tubes contained White's medium (*6*) which had been autoclaved with 1-percent agar. To this basic medium the following substances had been added after sterilization by filtration: (i) strong diffusate (1 ml of concentrated diffusate—diffusate from 50 seeds); (ii) weak diffusate (½ ml of concentrated diffusate plus ½ ml of distilled water—diffusate from 25 seeds); (iii) adenine sulfate, 40 parts per million; (iv) adenine sulfate, 40 parts per million, plus indoleacetic acid, 1 part per million; (v) indoleacetic acid, 1 part per million; (vi) distilled water, which served as a control.

Again the modified phenotype appeared when the reduced embryos were treated with either diffusate or adenine (Table 3). It is clear that a substance (or substances) which diffuses out of normal tomato seeds stimulates the development of cotyledon-like structures and buds in the reduced embryos. It is possible that the poorer results obtained at higher concentration of diffusate may be due to toxic substances in the diffusate or to an imbalance of factors. Adenine appears to give the full effect of the diffusate, and its effectiveness is not enhanced by indoleactic acid. Embryos which develop in response to either adenine or diffusate continue to grow when transferred to soil and produce a plant very similar to narrow; that is, the phenotype modified is a phenocopy of narrow.

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5 October 1959

Mitotic Arrest by Deuterium Oxide

Abstract. In marine invertebrate eggs, where cell divisions occur without growth, deuterium oxide produces arrest of, or serious delay in, mitosis and cytokinesis. All stages requiring assembly or operation of mechanical structures in the cytoplasm are sensitive to D_2O. The block is reversible in some cells.

Recent investigations of the effect of heavy water on biological systems have emphasized a finding implicit in the early papers—that in concentrations as low as 30 to 40 percent, D_2O arrests or delays cell division, without serious or immediate effects upon growth. Newer experiments on mammals and algae, respectively, are reported by Katz *et al.* (*1*) and by Moses *et al.* (*2*).

This result is of interest for two reasons: first, because the "isotope effect" is so large (see *3*), and second, because interference with cell division by so "simple" an agent offers some possibility of analysis in terms of primary molecular events.

We have undertaken a study of the effect of D_2O on fertilization and cleavage in the eggs of the sea urchin, *Arbacia punctulata,* and the annelid, *Chaetopterus pergamentaceus.* This report is a descriptive summary of the findings. Marine eggs were chosen for this study in order to avoid a source of confusion present in the systems heretofore investigated—that is, simultaneity of growth and division. As explained by Swann (*4*), division occurs in the absence of growth in the early development of the sea urchin. The same is true, presumably, for the egg of *Chaetopterus.*

Lucké and Harvey reported in 1935 (*5*) that cleavage of sea-urchin eggs was completely blocked in 99.5 percent D_2O, while in the same cells there was no evidence of rapid changes in permeability. Hoberman *et al.* (*6*) have found that cleavage is somewhat delayed in sea water containing a 9.1 atom-percent excess of D. Ussing (*7*), studying amphibia, and hampered by the necessity of employing minute volumes of medium, was able to show that even low concentrations of D_2O retard cleavage, and that no cleavage takes place in D_2O concentrations higher than 40 percent.

The experiments under discussion were concerned with the following variables: effect on fertilization compared with that on division, concentration of D_2O, stage of arrest or delay, possibility of reversal, and species.

When sea urchin eggs were inseminated in sea water and in a series of sea waters reconstituted with D_2O, it was found that eggs in media containing up to 25 percent (*8*) of heavy water fertilized normally. The criterion for

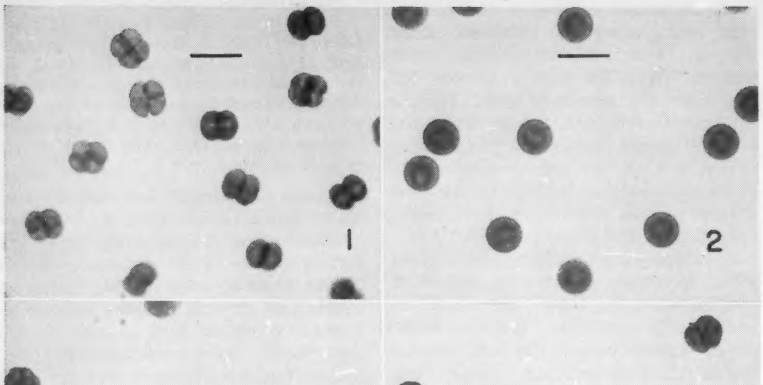

Fig. 1. *Arbacia* eggs (controls), 93 minutes after fertilization, showing four-cell stages. Fig. 2. Eggs from the same batch, placed in sea water containing D_2O 15 minutes after insemination, photographed at 96 minutes. Fig. 3. Controls 3½ hours after fertilization. Fig. 4. The same eggs as in Fig. 2, returned to normal sea water after 85 minutes in D_2O. Multiple irregular cleavages are evident. The horizontal lines represent 100 μ.

normal fertilization was the elevation of a clearly separated fertilization membrane within 5 minutes after insemination. At higher concentrations, the number of eggs fertilized declines sharply, so that in 50 percent D_2O, less than half of the eggs ever elevate membranes; in 75 percent D_2O, about 10 percent do so, and in 96 percent D_2O, 2 percent of the eggs are fertilized. Sperm motility seems to be unaffected, even in media containing 96 percent heavy water, for at least several minutes after dilution of the seminal fluid.

The effect of D_2O on cell division is separable from the effect on fertilization, as is shown in the following manner. Eggs were transferred from normal sea water, in which they had been fertilized, to sea water containing 96 percent D_2O, at 20, 30, 42, 47, and 50 minutes after insemination. Simultaneously, control samples were transferred to reconstituted sea water prepared from dried salts and distilled water. Mitotic events were followed by continuous observation of the cells and by photography of several low-power fields of each sample at intervals of about 5 minutes. All of the controls showed a normal sequence of events: the beginning of a "streak" at 20 minutes, metaphase spindles at 42 minutes, a few furrows beginning at 47 minutes (hence, anaphase), and normal cleavages at 50 minutes. The cells in D_2O were all arrested *at the stage of entry*. No cleavages (other than those already present in the 47- and 50-minute samples) were accomplished in these cells, even when the controls were entering the second division. Thus, it appears that the block occurs in all stages of mitosis and cytokinesis.

Sixty-six minutes after the immersion of each batch in D_2O, the experimental samples were washed with normal sea water. Their development was followed for several hours. Some reversal of the block was seen in all of the experimental samples; within 60 minutes after washing, all samples contained numbers of dividing cells. Some 30 to 40 percent of the treated cells never divided, but in the remainder, the divisions were frequently multiple and rapid, albeit irregular. There was a distinct tendency for treated cells to "catch up" with controls. This suggests that metabolic work and chromosome duplication were not blocked.

The photographs (Figs. 1–4) were made during another experiment in which the cells were immersed in D_2O fifteen minutes after insemination. Figure 1 shows the condition of the controls 93 minutes after fertilization, and Fig. 2, that of the D_2O-treated cells 96 minutes after fertilization. The blocked cells had formed no spindles, and the nuclear membrane had not broken down, but a curious mottling of the peripheral cytoplasm appeared. After 85 minutes in D_2O, the experimental cells were washed and returned to normal reconstituted sea water. Figure 3 shows the controls at 3½ hours after fertilization, and Fig. 4 shows the condition of the washed eggs at the same time. Evidently, a significant reversal of the block had been effected.

Experiments with lower concentrations of D_2O show that the effect of D_2O on division is more severe than the effect on fertilization. In sea water containing 75 percent D_2O, division remains completely blocked; in 50 percent D_2O, many eggs remain blocked and some cleave, but only after a delay of from three to four times the normal cleavage interval. Only below a concentration of 10 percent does the interference with cell division disappear.

Similar results were obtained with *Chaetopterus* eggs—that is, a 95-percent concentration of D_2O blocks cytokinesis and mitosis at all stages beyond early prophase. In this form, the number of cells ultimately dividing after removal from D_2O is much smaller than in *Arbacia*, but a degree of reversibility of the effect is evidenced by the dramatic disappearance of the gross shrinkage and "blebbing" seen during the D_2O treatment.

Thus it appears that the formation of normal mitotic spindles and cleavage furrows is impossible in concentrations of D_2O higher than about 75 percent. The data suggest that the primary effect is on processes associated with the mechanical structures of the dividing cell and on the assembly of these structures. This would imply much less interference with the metabolic events preceding, and necessary to, mitosis. The implication is supported by the observation of multipolar cleavages in the cells removed from D_2O after a long immersion. One might suppose the site of the isotope effect to be a large, extensively hydrogen-bonded structure, possibly the forming or complete mitotic apparatus itself (*9*).

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Nonlogarithmic Linear Titration Curves

Abstract. Titration curves can be based on linear nonlogarithmic forms of the equilibrium equation of a dissociation reaction. From such curves, in contrast to those based on logarithmic transformations, both the end point of the titration and the dissociation constant can be derived.

In recent articles in *Science* (*1*), graphic methods, based on a linear logarithmic transformation of the equation $(X) \cdot (Y)/(XY) = K$, have been recommended for the purpose of estimating the kinetic constants of dissociation reactions of the type $X + Y \rightleftharpoons XY$. However, the equilibrium equation can also be written in several nonlogarithmic forms, one of which is:

$$(P) = (X) + (XY) = (XY) + K \cdot (XY)/(Y) \qquad (1)$$

Thus, when X is titrated with Y, a plot of (XY) versus $(XY)/(Y)$ gives a straight line. The intercept with one of the coordinates equals P (the end point of the titration), while K is given by the slope. Linear logarithmic plots do not give the end point of the titration, which must be known for the construction of the curves. This is a disadvantage in cases where the concentration of the compound to be titrated is unknown.

Equation 1 is applicable to any dissociation reaction where the (relative) concentrations of XY and that of one of the dissociation products can be measured. For instance, it applies to the "titration" of an enzyme with its substrate, where the "end point" is most often unknown.

For this purpose, three nonlogarithmic linear equations were first suggested by Woolf (see *2*). The equation analogous to Eq. 1 and, for various reasons (*3*) to be preferred to the other two, can be written as:

$$(E_t) = (E) + (ES) = (ES) + K_M \cdot (ES)/(S)$$

or:

$$V_m = v + K_M \cdot v/(S)$$

where v (the initial reaction rate) equals $k \cdot (ES)$ and V_m (the maximal rate for $(S) \rightarrow \infty$ or the "end point of the titration") equals $k \cdot (E_t)$. Since, owing to low enzyme concentration, the concentration of free substrate is practically equal to the total substrate concentration (S), a plot of v versus $v/(S)$ is linear. Extrapolation to $(S) \rightarrow \infty$ gives V_m, while K_M (the Michaelis constant) is given by the slope.

A further example is the titration of the salt of a weak acid (AH) with a strong acid. From

$$(A^-) \cdot (H^+)/(AH) = K_H$$

it follows that

$$P = (A^-) + (AH) = (AH) + K_H \cdot (AH)/(H^+)$$

P is found by extrapolation of a plot of (AH) versus $(AH)/(H^+)$ to $(H^+) \rightarrow \infty$. K_H is derived from the slope of the plot.

When the sodium salt of AH is titrated withHCl, one has on the basis of electroneutrality (*4*):

$$(A^-) + (Cl^-) + (OH^-) = (Na^+) + (H^+)$$

Furthermore

$$(A^-) + (AH) = (Na^+)$$

Thus

$$(AH) = (Cl^-) - (H^+) + (OH^-)$$

where (Cl^-) can be expressed in equivalents of added acid.

In Fig. 1 such titration curves are given for 10 ml of $10^{-2}M$ solutions of Na_2HPO_4 and of the sodium salt of diethylbarbituric acid (Veronal) titrated with $1N$ HCl by means of a micrometer and syringe with capillary tip. The pH measurements were carried out with a Radiometer pH-meter standardized with a buffer based on U.S. National Bureau of Standards certified buffers.

Since the smallest increment of HCl added constituted a concentration of (Cl^-) of $10^{-3}M$ in the reaction mixture, (H^+) and (OH^-) could be neglected over the entire range of pH values ($\sim$ 6 to 9) used, and the equivalents (T) of added HCl were simply plotted versus $T/(H^+)$. The slopes of the curves correspond to pK_H values of 7.02 and 7.90 (30°C, 0.01M ionic strength) for phosphate and diethylbarbituric acid, respectively.

When a solution of the acid AH is titrated with a strong base, for example NaOH, one finds that:

$$(AH) = (P) - (Na^+) - (H^+) + (OH^-)$$

that is, for a plot of (AH) versus $(AH)/(H^+)$, the end point must be known. However, since Eq. 1 can be written as:

$$P = (A^-) + (A^-) \cdot (H^+)/K_H$$

a plot of (A^-) versus $(A^-) \cdot (H^+)$, where $(A^-) = (Na^+) + (H^+) - (OH^-)$, gives P as well as K_H, also in this case. The equations can readily be adapted to the case of a weak base.

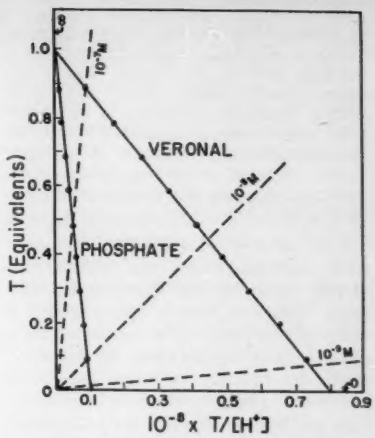

Fig. 1. Nonlogarithmic linear titration curves of phosphate and diethylbarbiturate (Veronal). T represents equivalents of HCl added to a solution of the sodium salt. $[H^+]$ is the hydrogen ion concentration. Points on the curves referring to a particular hydrogen ion concentration lie on a line through the origin. The ordinate represents $[H^+] = \infty$ and the abscissa $[H^+] = 0$. The intercept of the curves with the ordinate gives the end point of the titration, while the (negative) slope is equal to the hydrogen ion dissociation constant.

This method of plotting titration data has the further advantage over linear logarithmic plots of being more sensitive to experimental errors and small deviations from theory. This is emphasized by the fact that for K_H values that differ by more than one pK unit, the scale of the plot must be adapted to each constant separately. On the other hand, a more detailed analysis of a composite titration curve, involving two or more K_H values that are less than one unit apart, can be made on the same scale by using the procedure described here (*5*).

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20 July 1959

On the Function of Corticothalamic Neurons

Abstract: The effect of the synchronous discharge of a large population of corticothalamic neurons on activity within the somatosensory relay nuclei has been studied. Thalamic responses to peripheral nerve stimulation are depressed by activity in corticothalamic neurons. A subconvulsive dose of strychnine, given intravenously, changes this depression to enhancement.

The existence of corticothalamic neurons projecting from the cortical receiving areas to the specific thalamic relay nuclei has been recognized for many years, yet little is known of their physiological significance. In order to study the function of these cells it is necessary to activate them in large numbers and synchronously, yet selectively, without firing the nearby thalamocortical neurons antidromically. This may be achieved through the application of penicillin to the cortex as a means of initiating a repetitive high-voltage cortical discharge, the "penicillin spike." It is the purpose of this communication to describe marked alterations in the amplitude of thalamic-evoked responses elicited after a penicillin spike. Forty-seven cats deeply anesthetized with Nembutal were used in this study.

After stimulation of the somatosensory pathway it is possible to record an evoked response from the ventrobasal thalamus: a brief, initially positive, diphasic spike followed by a small negative and a large, slow positive wave. If this thalamic response is initiated during or after a cortical penicillin spike, the negative and late positive waves are greatly reduced or abolished, provided the penicillin spike was generated in the specific cortical receiving area of the nucleus from which the recording was made. Thus, the application of penicillin to somatosensory cortex I generates penicillin spikes which markedly depress the ventrobasal thalamic responses to stimulation of the sciatic or radial nerve or the medial lemniscus. On the other hand, penicillin spikes generated in the auditory or visual cortex have no effect on the responses evoked in the ventrobasal thalamic nuclei.

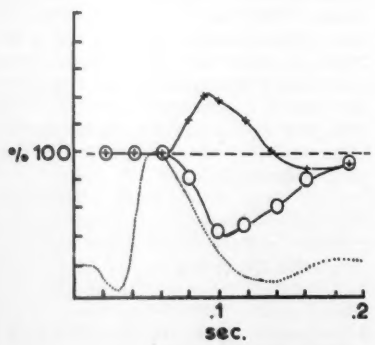

Fig. 1. Effect of the penicillin spike on the ventrobasal thalamic response to stimulation of the medial lemniscus before (circles) and after (crosses) intravenous administration of 0.1 mg of strychnine per kilogram. (Vertical axis) Average amplitude of the late positive wave from 50 thalamic responses represented as a percentage of the control value. (Horizontal axis) The penicillin spike (dotted line) diagrammatically displayed against time.

An important clue to the mechanism by which a cortical discharge may depress thalamic activity is seen in the effect on such activity of a subconvulsive dose of intravenous strychnine. Three to four minutes after intravenous administration of 0.1 mg of strychnine per kilogram, the penicillin spike, rather than depressing the thalamic-evoked response, actually enhances the late positive wave. This is illustrated in Fig. 1. The amplitude of the late positive wave of the ventrobasal thalamic response to medial lemniscal stimulation is represented on the vertical axis as a percentage of the control value. Each point represents the average of 50 responses. On the horizontal axis the cortical penicillin spike in shown diagrammatically (dotted line). It may be seen that before the administration of strychnine the thalamic test response is greatly depressed when it is elicited after the peak of the cortical penicillin spike (circles). Five minutes after the intravenous administration of 0.1 mg of strychnine per kilogram, the depression is abolished and the thalamic response is greater than for the control (crosses). Neither the penicillin spike itself nor the thalamic-evoked response alone is affected by the strychnine.

The possibility that the thalamic depression is caused by activation of the brainstem reticular formation (see *1*) is considered unlikely because of the depth of the anesthesia used. A second possibility, that the thalamic depression is due to antidromic backfiring of thalamocortical neurons, is also considered extremely unlikely because of the action of the strychnine in reversing the depression. Strychnine, in the amount used here, has been shown to be very effective in blocking inhibitory synapses in the spinal cord of the cat (*2*). Since synapses would not be involved in thalamic depression caused by antidromic activation of thalamocortical neurons, it is inferred that the depression is in fact mediated trans-synaptically through corticothalamic neurons. A reasonable hypothesis to explain the above observations is that the penicillin spike activates corticothalamic neurons with both excitatory and inhibitory thalamic synapses, the latter being predominant. The strychnine, in selectively blocking the inhibitory synapses, abolishes the depression of thalamic activity which they cause and permits expression of the previously masked, less potent excitatory activity. It is suggested that the corticothalamic projection system may provide a form of inhibitory sensory feedback which could function to reduce the subliminal fringe about a zone of maximal excitation in the thalamus. This might serve to distinguish the boundaries between one such zone and another and provide a form of sensory "focusing" (*3*).

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14 August 1959

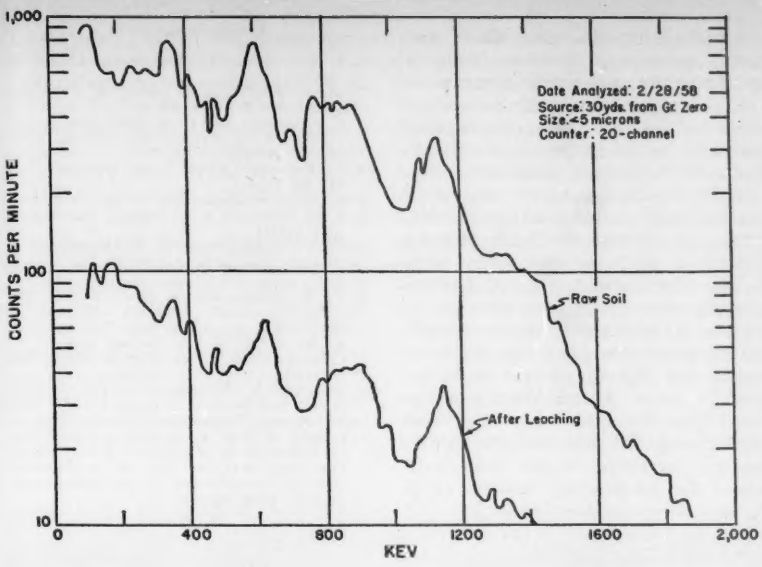
Occurrence of Scandium-46 and Cesium-134 in Radioactive Fallout

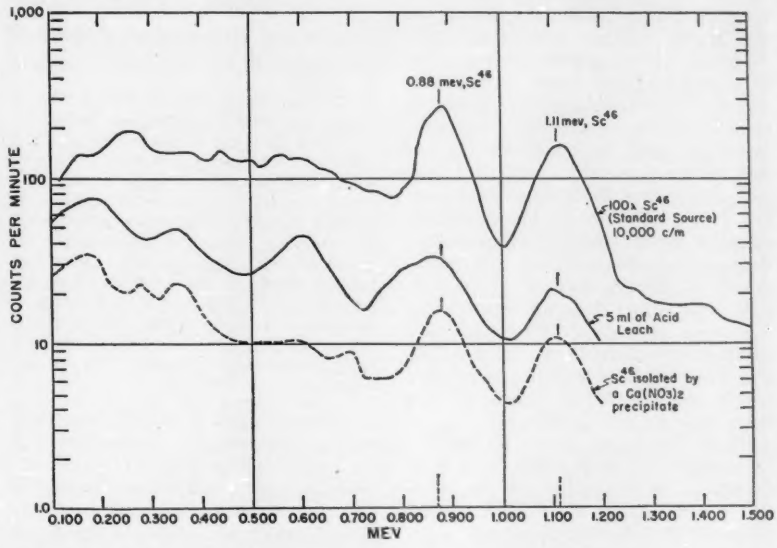
Abstract. Two hitherto unreported induced radionuclides, scandium-46 and cesium-134, have been detected in fallout material. Identification was made by chemical separation and gamma scintillation spectrometry. While the origin of these materials is not known, possible routes of formation from stable elements are suggested.

Soil samples (*1*) collected a few yards from ground zero by vacuum sweeping after an atomic detonation (balloon shot) in Nevada were sent to the Robert A. Taft Sanitary Engineering Center for radiochemical analyses.

An analysis of the gamma spectrum of an aliquot of the soil was made approximately 60 days after collection. After digestion and leaching of activity presumably fused on the soil, an aliquot of the leach solution was simultaneously analyzed. The gamma spectra are shown in Fig. 1; they indicate the presence of gamma emitters whose energies are near 0.6, 0.8 to 0.9, and 1.1 Mev, respectively. According to data calculated by Hunter and Ballou (*2*), the predominant fission-product contributors to the total activity 60 days after detonation should be Zr^{95}-Nb^{95}, Ce^{141}, Y^{91}, and Sr^{89}. Since none of these nuclides has gamma energies corresponding to the photopeaks observed, analyses of the important fission products for specific radionuclides were performed.

After separation (*3*) of the major fission products—strontium, cesium, zirconium-niobium, and rare earths—we were unable to assign the 0.9 and





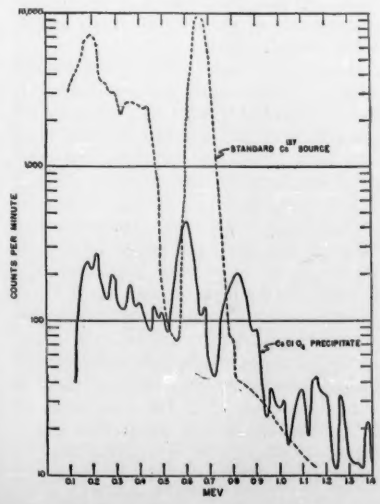


Fig. 1 (top). Gamma spectra for fallout mixed with water and acid leach. Fig. 2 (middle). Gamma spectra for activity in standard Sc^{46} source, in acid soil leach, and in chemically isolated Sc^{46} from soil collected 30 yd from ground zero. Fig. 3 (bottom). Comparison of gamma spectra of standard Cs^{137} source and of final $CsClO_4$ precipitate.

the 1.1 Mev gamma energies, or to account for all the activity present. Manganese-54, iron-59, and zinc-65, previously reported in fallout, were eliminated as possibilities after radiochemical analyses to obtain each of these gamma emitters in a purified form. Eventually, the possibility that both peaks might be attributable to Sc^{46} was suggested (*4*).

Attempts to separate scandium by reported methods (*3*,*5*) failed, possibly because yttrium was used as a carrier in the absence of stable scandium. It was found, however, that most of the gamma activity at 0.9 and 1.1 Mev was carried on $Ca(NO_3)_2$ precipitated by fuming nitric acid. Known Sc^{46} tracer was also carried by this treatment. This purification step permitted identification of the unknown emitter as Sc^{46} by its gamma spectrum. The spectra, shown in Fig. 2, show the agreement with the known Sc^{46} photopeaks at 0.88 and 1.11 Mev. (*6*). The Sc^{46} content, estimated from the area under the photopeaks, accounted for approximately 40 percent of the total activity leached, or for most of the previously unidentified activity.

The cesium fraction, which had been carefully purified (*7*), showed prominent gamma peaks at 0.6 and 0.8 Mev, with only a slight indication of the expected peak at 0.66 Mev. In Fig. 3 the spectrum of a standard Cs^{137} source is compared with that of the $CsClO_4$ precipitate, and the difference in the photopeaks is apparent. The 0.6 and 0.8 Mev peaks correspond to those of Cs^{134} (*6*).

Without knowledge of the past history of this soil, it is difficult to explain the presence of Sc^{46} and Cs^{134}. Since no soil from this area had been chemically analyzed prior to this particular test, information on its composition is not available, specifically with respect to the presence of stable scandium or cesium. If it is assumed that these activities were induced during a nuclear explosion, possible reactions for production of Cs^{134} would be:

$$Cs^{133}\ (n, \gamma)\ Cs^{134}$$
$$Ba^{134}\ (n, p)\ Cs^{134}$$
$$Ba^{136}\ (n, t)\ Cs^{134}$$

Similarly, Sc^{46} might be produced by the reactions:

$$Ti^{46}\ (n, p)\ Sc^{46}$$
$$Sc^{45}\ (n, \gamma)\ Sc^{46}$$
$$Ti^{49}\ (n, t)\ Sc^{46}$$

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Degree of Obesity and Serum Cholesterol Level

Abstract. No significant correlation was found between the serum cholesterol level and weight, weight corrected for frame size, or thickness of the fat shadow in medical students (mean age, 22 years).

Among the variables thought to affect serum cholesterol levels in normal subjects, the degree of obesity has been of particular interest because the amount of stored fat is accessible to dietary control. The literature, however, is divided on the relationship of body fat to cholesterol level. Moreover, reports have been variously based on anthroposcopic "endomorphy" ratings (*1*, *2*), weight or relative weights (*3*), indices of body build (*4*), or infrascapular fat folds (*1*, *5*) and not, to our knowledge, on direct radiographic measurements of the outer-fat shadow.

In the study reported here we compared the thickness of the fat-plus-skin shadow, measured at the level of the tenth rib on standard posteroanterior teleoroentgenograms (*6*), with serum cholesterol levels, determined, by a modification of the Bloor method (*7*), in blood samples obtained at the time the roentgenograms were made. Less direct measures of relative obesity included weight, and weight expressed in relation to the bony-chest diameter, as measured on the films. Replicability was 0.95 for the fat measurements (*8*) and 0.92 for serum cholesterol (*7*).

Table 1. Correlations between various measures of obesity and total serum cholesterol in healthy young men.

N	Mean body wt. (kg)	Serum cholesterol (mg/100 ml)*	Correlation (*r*)
		Body weight	
159	74.8	225.2±36.3	0.033
		Body weight/chest breadth	
134	73.4	226.0±37.8	0.126
		Lower thoracic fat	
125	73.2	222.5±34.2	−0.030

* Mean ± standard deviation.

Subjects included the 159 white male medical students in the classes of 1958, 1959, and 1960 of the Johns Hopkins School of Medicine, for whom body weight, cholesterol level, and posteroanterior chest plates were obtained at the time of admission. The mean height for the group was 178.9 cm, the mean weight was 74.8 kg, and the mean age was 22 years. The cholesterol range was 140 to 386 mg/100 ml with a mean of 225 mg/100 ml, and the lower-thoracic fat range for the subjects in whom the fat-plus-skin shadow could be accurately measured was 2 to 15 mm, with a mean of 7.4 mm.

As shown in Table 1, the correlation between serum cholesterol level and weight in the series of 159 men was not significantly different from zero at the 5-percent level of confidence ($r = 0.13$). In a restricted subsample, from which subjects whose radiographs were not suitable for the chest-breadth measurement had been excluded, the correlation between serum cholesterol and weight corrected for build was similarly low ($r = 0.13$). Finally, the correlation between serum cholesterol and lower-thoracic fat, measured on 125 radiographs technically suitable for the purpose, was −0.03. It is noteworthy that the mean cholesterol levels and their standard deviations in the total sample and in the partial samples were very nearly the same. The ranges for the three groups were identical.

There are many factors in addition to nutritional status which have been shown to be related to the height of the serum-cholesterol level of healthy persons—in particular, age, sex, race, heredity, endocrine patterns, habits of smoking and exercise, and degree of emotional stress. We have reported positive correlations between several of these factors and high cholesterol levels among the Johns Hopkins medical students (*9*). The lack of relationship between the amount of body fat and the serum cholesterol level demonstrated in the present study supports the view that the nutritional status of healthy young men such as medical students is considerably less important in determining the cholesterol level than other biologic factors (*10*).

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19 August 1959

Neural and Hypophyseal Colloid Deposition in the Collared Lemming

Abstract. Feral and captive lemmings from Churchill, Manitoba, are subject to a unique pathological process in which a colloidal material is deposited in blood-vessel walls at scattered points through the central nervous system. Destruction of nervous tissue at these foci is progressive, and colloidal masses in the vascular lumina of the hypothalamus appear to become fixed in the capillaries of the hypophyseal anterior lobe. Inflammatory reactions are never associated with the lesions, and the latter are larger and more numerous in older animals in warmer environments.

A current study of the microanatomical and physiological characteristics of the collared lemming (*Dicrostonyx groenlandicus* Traill) has revealed a unique and previously unknown pathology which is probably a significant factor in the behavior and population fluctuations of these ecologically significant arctic rodents.

Lemmings used in this study consist both of animals fixed in 10-percent neutral buffered Formalin immediately after capture at Churchill, August 1953 and July 1954, and captives and their progeny perfused and fixed in Bouin's fluid. Observations on the behavior, reproduction, growth, diseases, and reproductive and endocrine organs of these animals have been presented (*1*). Captive lemmings were raised and maintained usually in an artificially and constantly lighted room at 24°C (21° to 27°. Groups of animals were periodically transferred to darkness and a temperature of 23°C for 4 months, or to a thermostatically controlled deep-freeze cabinet with a transparent lid and forced circulation of air. After 10

to 14 days of decreasing temperature the lemmings were kept at −9° to −6° C, either in light for several days or in darkness for up to 4¾ months in apparent good health. Three animals were subjected to temperatures of 32° to 33°C for 1 to 2 days. At autopsy, weight, blood counts, and smears were made.

Serial paraffin sections of the brains of 16 of the best-preserved captives, which appeared to be healthy externally, were stained by a variety of methods (*2*) and demonstrate a pathological process without any signs of disease organisms or inflammatory responses (*3*). The characteristic and progressive lesions may be somewhat arbitrarily divided into five types: (i) segments of capillaries and venules with thickened endothelia containing granules staining with azure, periodic-acid Schiff, and Casella techniques; (ii) segments of capillaries and venules with similarly staining fibrous or granular deposits radiating into the surrounding neuropil as well as forming aldehyde fuchsin- and periodic-acid Schiff-positive spheroidal and amorphous intravascular colloid masses (Fig. 1,*A*); (iii) small (5 to 20μ) colloid bodies staining with aldehyde fuchsin and Bauer techniques and scattered in the neuropil, especially in the white matter of cerebellum and tegmentum, and probably resulting from lesions of types i and ii; (iv) spheroidal and amorphous colloid bodies detached in the ventricles of the brain and in veins, and originating at least in part from type ii lesions (Fig. 1,*B*); and (v) focal lesions of the type ii category but expanded to 120 to 160μ in diameter and containing centrally a metachromatic (Giemsa stain), aldehyde fuchsin-positive, insoluble lipid-containing material. Lesions of types i to iv are most numerous in lemmings in higher environmental temperatures, with the exception of certain of the oldest of the cold-treated females, which have many of these lesions also. Type v lesions are usually found only in animals over 6 months old. Brain lesions similar to some of these but generally less severe have been observed in lemmings fixed at capture.

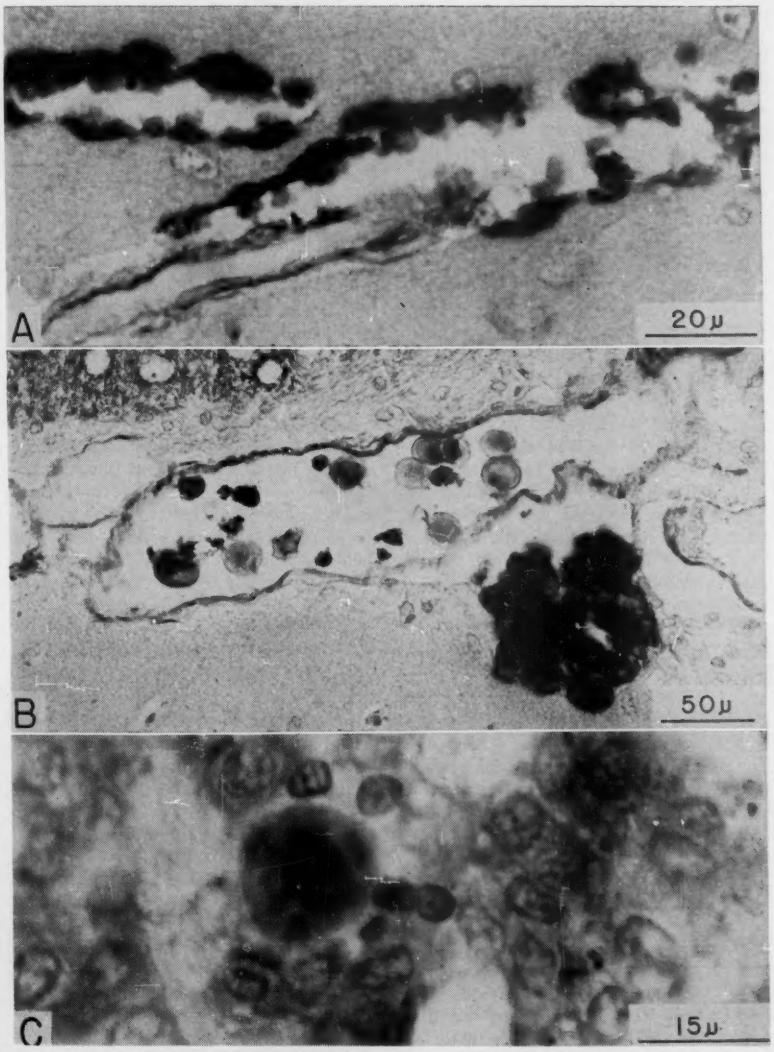

Fig. 1. (*A*) Type ii lesion spreading from the endothelia of a cerebral vessel (Casella technique and hematoxylin stain). (*B*) Detached and invasive colloid bodies in meningeal vessels (Oil red O and hematoxylin stain). (*C*) Hypophyseal colloid body in a feral lemming (periodic-acid Schiff and hematoxylin stain).

The anterior lobes of the hypophyses of all of the captives contain intravascular colloid bodies staining with aldehyde fuchsin and resembling those of the type iv brain lesions. When these are counted along with the parenchymal cells of the pars distalis in sections 7μ in thickness, they are found to form 0.7 to 4.5 percent of the elements at environmental temperatures of −9° to 6°C, 2.0 to 6.0 percent at 21° to 27°C, and 3.7 to 7.8 percent at 32° to 33°C. All but three of the 25 animals fixed at capture at Churchill show similar hypophyseal colloid bodies (Fig. 1,*C*), but usually less abundantly than the captive series at 21° to 27°C. It appears likely that the hypophyseal colloidal emboli descend via the hypothalamo-hypophyseal portal system, since they are not found in the hypophyseal posterior lobe, and formative stages are not seen in hypophyseal endothelia.

The natural occurence of this pathology in lemmings at Churchill, one of the most southern points in the geographic range of this species, and the augmentation of the lesions in moderately warm environments is interesting in terms of evidence for a metabolic limitation in these animals which may restrict their dispersal and influence their vigor and reproductive potential. It appears that adaptations for life in an arctic climate in these animals are associated with a peculiar vulnerability in warm environments (*4*).

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4. The early phases of this work were done in the department of anatomy, University of Michigan, and were aided by a grant from the Horace H. Rackham School of Graduate Studies.

16 October 1959

Meetings

Chemical Thermodynamics Discussed at Symposium Held near Innsbruck, Austria

A symposium on chemical thermodynamics was held 20–25 August at Fritzens-Wattens, near Innsbruck, Austria, under the joint sponsorship of the Deutsche Bunsen Gesellschaft für Physikalische Chemie and the International Union for Pure and Applied Chemistry, the latter through its Commission on Chemical Thermodynamics, its Subcommission on Experimental Thermochemistry, and its Subcommission on Experimental Thermodynamics. The symposium was under the general direction of K. Schäfer (president of the Deutsche Bunsen Gesellschaft für Physikalische Chemie and secretary of the Commission on Chemical Thermodynamics), and the local arrangements were handled by E. Cremer of the University of Innsbruck. Fifty papers were read, and there were 135 participants representing 17 countries; 30 were from North America.

Thermochemistry

In the first part of the symposium, on "Experimental Thermochemistry," 23 papers were presented. Eight of these were studies in bomb-combustion calorimetry. The most novel paper of this group, presented by W. N. Hubbard (Argonne National Laboratory) gave a preliminary account of the successful development of a bomb calorimeter for combustions in an atmosphere of fluorine gas. The bomb is made of nickel, which is rendered impervious to attack during the combustion process by a careful prefluorination treatment so that the inner surfaces become coated with a stable nickel fluoride film. Combustion experiments of an exploratory character have been made on 25 metals and 15 inorganic compounds, and three-quarters of these appeared to be immediately suitable for accurate calorimetric study.

Two papers described recent studies in which the rotating-bomb calorimeter is used. The bomb is charged before combustion with a suitable solvent and rotated after combustion to produce a uniform solution of the combustion products in a definite thermodynamic state. The method has been developed over the last few years and has made it possible to obtain highly accurate combustion data for a variety of organic sulfur-containing compounds. D. W. Scott (Bureau of Mines, Bartlesville, Okla.) described the further application of the technique to organic fluorides and to some organometallic compounds. The results obtained on 15 organic fluorides were presented, and values were reported for the heats of formation of lead tetramethyl, lead tetraethyl, and dimanganese decacarbonyl. The application of the method to organic bromides was described by L. Bjellerup (Lund, Sweden), and the derived heats of formation were critically compared with those independently available from heats of bromination. The values for heats of formation from combustion compare satisfactorily with those from reaction heats measured by Lacher and his associates at the University of Colorado.

The advantages of Polythene ampoules over glass ampoules as containers for liquids in combustion calorimeters were discussed by H. Mackle (Belfast). In the discussion it developed that several investigators have experimented with plastic ampoules with considerable success, and that Polythene and polyesters are suitable materials. Three papers reported new combustion studies by conventional "static-bomb" procedures: J. D. Cox (National Chemical Laboratory, Teddington) presented results on phenol and three cresols; M. Colomina (Madrid) gave the results of an investigation of five alkylphenylketones and of the three toluic acids; and J. Tjebbes (Lund) reported on the heats of combustion of butanal and five related compounds. In these studies, the limits of error were of the order of 0.01 to 0.02 percent; this is fairly representative of the degree of precision attainable by modern combustion calorimetric techniques in favorable cases. C. T. Mortimer (University College of North Staffordshire, Keele) reported heat of combustion data on triphenylphosphine, triphenylphosphine oxide, and triethyl phosphate. These compounds can hardly be described as "favorable cases" for combustion study, in view of the difficulty of analyzing the products of combustion, so the limit of error of only 0.1 percent reported by Mortimer is encouragingly small and suggests that further thermochemical studies on organophosphorus compounds of biochemical importance should be rewarding.

Reaction calorimetry (heats of reactions other than combustion) was represented by three papers. E. J. Prosen (Washington, D.C.) gave an account of the extensive studies at the National Bureau of Standards on the thermochemistry of boron hydrides, in which measurements have been made of the heats of thermal decomposition of the boron hydrides into elements, of their heats of hydrolysis, and of their heats of chlorination. The heat of formation of BCl_3 from elements has also been measured. Reliable values are now available for the heats of formation of the boron hydrides and of boric acid. Values were derived for the bond-energy terms of various boron bonds, including the tricentric "bridge bonds" involving boron atoms. The empirical scheme proposed by Prosen was applied to interpret structural features of the more complex hydrides of boron. H. A. Skinner (University of Manchester) also described some thermochemical studies on reactions of diborane—in this case the addition reactions of diborane with olefines to form boron trialkyls. These reactions occur rapidly in Diglyme solvent at room temperature and provide a promising route to determination of the heats of formation of the boron trialkyls. N. N. Greenwood (University of Nottingham) described the application of solution calorimetry to the measurement of heats of formation of 1:1 and 1:2 donor-acceptor complexes of $GaCl_3$, $GaBr_3$, BCl_3, and BBr_3 with various oxygen and nitrogen donors. For a given ligand, the heats of formation of the tribromide complexes are consistently greater than for the corresponding trichlorides, and the heats of formation of BCl_3 with nitrogen donors are larger than those of $GaCl_3$ with the same ligands.

Two papers dealt with the thermochemistry of alloys. O. Kubaschewski (National Physics Laboratory, Teddington) reviewed calorimeters suitable for measuring the formation heats of alloys, with particular reference to calorimeters developed at the National Physics Laboratory for direct measurments on solid alloys formed by heating powdered metal mixtures at high temperatures. A. Schneider (Göttingen) reported directly measured heats of formation of InSb, $MgZn_2$, MgZn, and Mg_2Zn_{11}, and indirectly determined free energies of formation of Fe_2Zr, Al_2Zr, Al_3Zr_2, and Al_3Zr_4.

There were three contributions concerned with the heats of mixing of liquids. In one of these, M. L. McGlashan (Reading University) emphasized that measurements of heats of mixing should be carried out under conditions in which the vapor phase is entirely eliminated;

he described an ingenious calorimeter for this purpose. Heats of mixing of pairs of compounds from the series $SiCH_3)_{4-n}Cl_n$ and from the series $C(CH_3)_{4-n}Cl_n$ were reported, and discussed in terms of the "quasi-chemical approximation." Mixing heats in the binary and ternary systems formed from acetic acid, pyridine, and a normal paraffin hydrocarbon (hexane, heptane, and so on) were reported by W. Woycicki (Warsaw). W. Wagner (Dresden) described a series of measurements of activity coefficients, heats of mixing, and volumes of mixing of the chlorotoluenes in benzene solvent.

Studies of chemical equilibria were the subject of two papers. In one of these, S. Sunner (Lund) described the use of the gas-phase chromatographic technique to analyze the disproportion reaction $MeSSMe + EtSSEt \rightleftharpoons 2MeSSEt$, at 25° and 60°C. A. Pacault (Bordeaux) spoke of a series of equilibrium studies on double decomposition reactions in the solid state between $BaSO_4$ and Na_2CO_3 and K_2CO_3.

The thermochemistry of Tl_2CO_3, Tl_2CS_3, and $BaCS_3$, determined from dissociation pressures measured over a temperature range, was described by G. Gattow (Göttingen), and the formation and thermodynamic stability of mixed halides (for example, CuClBr) were discussed by R. Perret (Dijon).

I. Wadsö (Lund) described a novel adiabatic calorimeter for the direct measurement of heats of vaporization at 25°C, suitable for liquids having a vapor pressure of from 5 to 100 mm-Hg at room temperature. The accuracy achieved was of the order of 0.5 percent.

The accepted value for the heat of dissociation of N_2 was questioned by G. Glockler (Durham, N.C.) and L. H. Long (Exeter University) raised the interesting problem of the relation between bond dissociation energies and valence-state excitation energies by specific reference to experimental data available on metal-carbon bonds.

This first part of the symposium was summed up by F. D. Rossini (Pittsburgh) and by J. Coops (Amsterdam). Rossini, reflecting on 30 years of thermochemistry, recalled the stages of development in this field: first, design of calorimeters of sufficient accuracy; second, preparation of compounds of sufficient purity; and now, extension of the range of investigation to substances more difficult to prepare, to purify, and to analyze, through the use of all the tools of modern analysis.

Chemical Thermodynamics

In the second part of the symposium, arranged by the Commission on Chemical Thermodynamics, six papers were presented. The first of these, given by Rossini, concerned the present state of knowledge on the fundamental constants of physical chemistry. Five of these have to be determined by experiment; the others are defined by international agreement. In addition, there are auxiliary constants which derive from the fundamental constants. Since 1951, the ice point of water has become a defined constant, and improved values have become available for the constants h, c, e, F, and N. It is proposed that, in 1960, a new scale of atomic weights based on $C^{12} = 12.0000$ be adopted; this differs from the present chemical scale by 42 parts per million, and from the present physical scale by 317 parts per million.

There were three papers on the thermodynamic scale of temperature and its realization through the international scale, given by Van Dijk (Leiden), H. Moser (Braunschweig), and C. Tingwaldt (Berlin).

In the range 0° to 90°K, new measurements have been made on the boiling points of helium and oxygen, with results which differ by only 0.001° and 0.01°, respectively, from the older values of 1937 and 1927. The values 4.215° and 90.18°K (relative to an ice point of 273.15°K) for the boiling points of helium and oxygen may be regarded as in practically complete agreement with the thermodynamic scale. For n-H_2, the most recent measurements of the boiling point by various investigations give the mean value 20.379°K. For secondary fixed points in this region there are: argon, boiling point 83.81°K; nitrogen, boiling point 77.35°K; oxygen, triple point, 54.34°K; p-hydrogen, boiling point 20.27°K; n-hydrogen, triple point, 13.95°K; p-hydrogen, triple point 13.81°K; helium, λ-point, 2.172°K.

In the range between the two fixed points of the boiling point of oxygen (−182.97°C) and the boiling point of sulfur (440.60°C), which is spanned in the international scale by measurements based on the platinum resistance thermometer, the deviations ΔT ($= T_{\text{thermodyn.}} - T_{\text{internat.}}$) vary from + 0.04° at −80° to <0.01° between 0 and 100° to + 0.13° at 400°C. In the range from the boiling point of sulfur to the melting point of gold, in which the international scale relies on the interpolation formula for the electromotive force of a Pt/Pt-Rh element, the deviations ΔT are as high as 1.7°C. It is suggested on the basis of new measurements that the melting point of gold is 1064.76°C rather than 1063°, as accepted in the international scale. This is a large change, and until more evidence is available, it is too soon to make the change in the international scale.

A change in the radiation constant from 1.438 to 1.4388, coupled with an increase of 1.76°C in the defined melting point of gold, would eliminate the differences between the international and the thermodynamic scales above 1500°K.

The measurement of temperatures above the gold point by optical pyrometry was discussed by C. Tingwaldt, with particular reference to experimental methods of obtaining ideal blackbody radiation from a glowing solid. The lateral radiation from a glowing metal is extensively polarized "nonblack" radiation; by allowing radiation from one source to fall at an angle of 45° onto a glowing metal strip, the combined radiation (reflected and emitted) from the strip is unpolarized, black radiation corresponding to the temperature of the glowing source.

Two papers were presented in which calorimeters of new design were described. One of these, by A. R. Meetham (National Physics Laboratory, Teddington) reported on the design and use of an aneroid bomb calorimeter, made of silver and vacuum-jacketed, for measuring heats of combustion. The heat-transfer coefficient of the calorimeter is very much smaller than that of stirred-water calorimeters. The internal volume is 97 ml, and 0.2 gm of benzoic acid on combustion gave a temperature rise of 3.5°C. The standard deviation of individual estimates of the heat of combustion of benzoic acid was less than 1 part in 5000.

F. E. Wittig (Munich) described the constructional details and operation of a high-temperature calorimeter for thermal studies on metallic alloys.

Phase Changes in Condensed Systems

The third part of the meeting, on "The Thermodynamics of Phase Changes in Condensed System," was sponsored by the subcommission on experimental thermodynamics. Introducing the first session, D. M. Newitt (Imperial College, London) said that the object of this meeting, and of its predecessor in London in 1957, was to encourage work to fill the gaps in our knowledge of thermodynamic properties, and especially in our knowledge of pure substances.

Most of the speakers discussed the phase transitions of solids. There were two introductory reviews. The first was by J. E. Mayer (Chicago), who gave a lucid account of the thermodynamics of phase changes in terms of the singularities of the Helmholtz free energy and pointed out how restricted are the types of transition that can be adequately treated by the present developments in statistical mechanics. The second was by J. P. McCullough (Bureau of Mines, Bartlesville), who outlined an empirical division into seven types of the solid-solid transitions of hydrocarbons and of related substances.

A. R. Ubbelohde and his colleagues E. Rhodes and E. R. Buckle (Imperial

College, London) contributed a group of three papers on the mechanism of melting and freezing. They demonstrated most convincingly that the classical thermodynamic picture of melting as the sharp intersection of two unrelated free-energy curves is inadequate for material composed of complex molecules or ions.

The largest group of papers in this section were detailed studies of particular solid systems. Silver oxide was shown by K. S. Pitzer (Berkeley) to have two anomalous regions in its heat-capacity curve. The first, at 20° to 40°K, is still without a verified explanation, but the second, at 370° to 470°K, is an annealing of surface and crystal defects. J. G. Aston (Pennsylvania State University) and E. F. Westrum (Michigan) both presented measurements of the heat capacities of systems containing highly symmetrical organic molecules—the *molecules globulaires* of Timmermans. Such systems are of particular interest since the symmetry and "smoothness" of the molecules allow them to melt with respect to their orientations without disrupting the crystal. This orientational energy and entropy is acquired at one or more transitions below the true melting point. J. E. Spice (Liverpool) reported a detailed study of mixtures of ethylene dichloride and dibromide. The former has long been known to have a broad maximum in its heat-capacity curve at 180°K. On dilution with dibromide the peak moves at first to lower temperatures and becomes less pronounced. The behavior of the system is, however, complicated by the partial immiscibility of the two solids.

The thermodynamic studies of indane and indene by D. R. Stull (Dow Chemical Co.) were most notable for the high degree of "automation" in the calorimeter. This led to many questions from an audience very conscious of the time it takes to reach equilibrium in many solid systems.

The magnetic transitions of the divalent salts of manganese, iron, cobalt, and nickel were reviewed by J. W. Stout (Chicago). These systems provided unusually elegant examples of the entropy ($R \ln n$) associated with n available electronic states per ion. W. E. Wallace (Pittsburgh) has made a very detailed study by calorimetry and by electrical, magnetic, and x-ray examination of the differences between the α and β phases of Ta_2H and Ta_2D. The results were interpreted in terms of differing amounts of short-range and long-range order in the allocation of the hydrogen atoms to the interstitial sites. G. M. Schwab (Munich) reported a study of the α-to-β phase change in cobalt.

The last group of papers was that which dealt primarily with liquid phases, or in which interest centered in the application of theories of mixtures to the results. R. Heastie (Queen Mary College, London) had studied the phase equilibria of mixtures of krypton with argon and with xenon. The systems were miscible in both solid and liquid phases at the melting point, and the form of the melting curves provided a test for the lattice theories of solution of Prigogine. Unfortunately, true equilibrium in the solid phases was not always achieved. T. M. Storvick and J. M. Smith (Northwestern University) have studied the thermodynamic properties of mixtures of hydrocarbons and alcohols in liquid and gaseous phases at temperatures up to the gas-liquid critical points. The deviation of their results from those for purely hydrocarbon mixtures was interpreted in terms of the degree and heat of polymerization of the alcohol molecules. J. S. Rowlinson (University of Manchester) demonstrated that lower critical solution points are not confined to polar mixtures but are commonly found also in binary hydrocarbon mixtures if the size ratio of the constituent molecules is sufficiently large. This behavior appears to be the rule for high polymer solutions, almost all of which separate into two phases above the normal boiling point of the solvent. Finally, R. I. Munn (Vienna) reported a careful reexamination of the phase boundary curve near the lower critical solution point of the system water and triethylamine. The curve has an unusual, and unexplained, point of inflection.

Three papers less easy to classify were those by K. S. Pitzer (Berkeley) on irreversible thermodynamics, by D. White (Columbus, Ohio) on *o-p* and isotope separations by preferential adsorption at low temperatures, and by G. Watelle-Marion (Dijon) on a spectrophotometric study of the ionization of a divalent metal salt.

GUY WADDINGTON
National Academy of Sciences–National Research Council, Washington, D.C.

Forthcoming Events

January

28–30. Mathematical Assoc. of America, 43rd annual. Chicago, Ill. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

28–30. Western Soc. for Clinical Research, 13th annual, Carmel-by-the-Sea, Calif. (W. N. Valentine, Western Soc. for Clinical Research, Univ. of California Medical Center, Dept. of Medicine, Los Angeles 24.)

29–30. American Group Psychotherapy Assoc., Inc., 17th annual conf., New York, N.Y. (American Group Psychotherapy Assoc., Inc., 1790 Broadway, New York 19.)

29–30. Host Influence on Parasite Physiology, New Brunswick, N.J. (L. A. Stauber, Rutgers Univ., New Brunswick.)

31–5. American Inst. of Electrical Engineers, New York, N.Y. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

31–7. Pan American Cong. of Ophthalmology, 6th, Caracas, Venezuela. (J. W. McKinney, 921 Exchange Bldg., Memphis, Tenn.)

February

1–4. American Soc. of Heating, Refrigerating and Air Conditioning Engineers, semi-annual, Dallas, Tex. (Miss J. I. Szabo, ASHRACE, 234 Fifth Ave., New York 1.)

1–4. Instrument-Automation Conf., Houston, Tex. (Director, Technical and Educational Services, Instrument Soc. of America, 313 Sixth Ave., Pittsburgh 22, Pa.)

1–5. American Inst. of Electrical Engineers, winter general, New York, N.Y. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

1–5. Clinical Cong. of Abdominal Surgeons, Miami Beach, Fla. (CCAS, 633 Main St., Melrose 76, Mass.)

2–4. Haemopoiesis—Cell Production and Its Regulation, Ciba Foundation symp. (by invitation only), London, England. (G. E. W. Wolstenholme, Ciba Foundation, 41 Portland Pl., London, W.1, England.)

2–4. Society of the Plastics Industry (Reinforced Plastics Div.), Chicago, Ill. (W. C. Bird, SPI, 250 Park Ave., New York 17.)

3–5. Military Electronics, IRE winter conv., Los Angeles, Calif. (G. B. Knoob, Motorola, Inc., Military Electronics Div., 1741 Ivar Ave., Hollywood 28, Calif.)

3–6. American College of Radiology, New Orleans, La. (W. C. Stronach, 20 N. Wacker Dr., Chicago 6.)

3–6. Parathyroid Research, symp., Houston, Tex. (R. V. Talmage, Dept. of Biology, Rice Inst., Houston.)

4–6. American Soc. for Metals, San Francisco, Calif. (R. Huggins, ASM, Stanford Univ., Stanford, Calif.)

4–6. Congress on Administration, 3rd annual, Chicago, Ill. (R. E. Brown, American College of Hospital Administrators, 840 N. Lake Shore Drive, Chicago 11.)

5. Parenteral Drug Assoc., New York, N.Y. (H. E. Boyden, PDA, 4865 Stenton Ave., Philadelphia 44, Pa.)

7–9. Congress on Medical Education and Licensure, Chicago, Ill. (CMEH, AMA, 535 N. Dearborn St., Chicago 10.)

7–10. Radioactive Isotopes in Clinical Medicine and Research, 4th intern. symp., Bad Gastein, Austria. (R. Höfer, 2nd Medical Univ. Clinic, 13 Garnisongasse, Vienna 1X, Austria.)

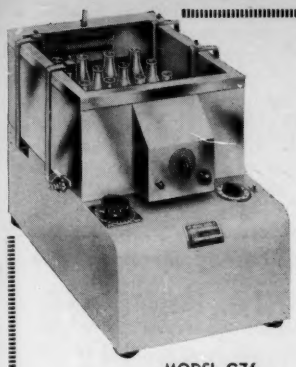
10–11. Gas Cooled Reactor, symp., Philadelphia, Pa. (F. L. Jackson, Franklin Inst., Philadelphia, Pa.)

10–12. American Acad. of Occupational Medicine, Williamsburg, Va. (L. B. Shone, Bureau of Medicine and Surgery, Navy Dept., Washington 25.)

10–12. Solid States Circuit Conf., Philadelphia, Pa. (T. R. Finch, Bell Telephone Laboratories, Murray Hill, N.J.)

10–13. National Assoc. for Research in Science Teaching, 33rd annual, Chicago, Ill. (C. M. Pruitt, Univ. of Tampa, Tampa, Fla.)

(See issue of 18 December for comprehensive list)



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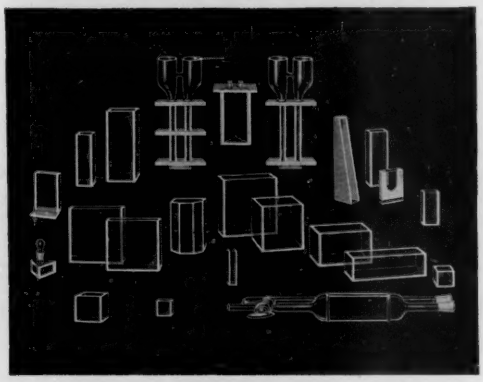

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■ TRANSISTOR TEST SET measures beta parameter from 0 to 30/100/300; input impedance from 500 to 20,000 ohms; and collector cut-off current from 0 to 50 μa. Test frequency is normally 1 kcy/sec from an internal oscillator. Internal collector voltage source is a 52-v mercury cell. Accuracy is said to be ±2 percent for cut-off current, ±3 percent for beta, and ±5 percent for input impedance. (Metronix, Inc., Dept. Sci264, Chesterland, Ohio)

■ CURRENT PULSE GENERATORS designed to furnish test pulses for computer components furnish 1-amp (max.) pulses with 0.1-μsec rise time. Separate models are available for positive and negative pulses. Width and pattern of pulses are determined by external logic circuitry. Current setting is continuously adjustable. (Digital Equipment Corp., Dept. Sci266, Maynard, Mass.)

■ MINIATURE WIRE-WOUND POTENTIOMETER has power rating of 1.0 watt and maximum operating temperature of 175°C. Dimensions are 3/16 by 5/16 by 1 in. Resistances from 100 to 20,000 ohms are available. The units are said to be impervious to salt spray, fungus, sand, and dust, as well as to exceed government specifications for shock, vibration, and acceleration. (Bourns Laboratories, Inc., Dept. Sci271, P.O. Box 2112, Riverside, Calif.)

■ RADIO-FREQUENCY VOLTAGE CALIBRATOR provides four output ranges between 2 mv and 3 v r.m.s. Accuracy is said to be ±0.3 percent of full scale, and maximum frequency effect is said to be ±0.2 percent from d-c to 10 Mcy/sec. The instrument requires a 3-volt input signal into 600 ohms at any frequency up to 10 Mcy/sec. A built-in d-c reference permits calibration of full-scale deflection. (Sensitive Research Instrument Corp., Dept. Sci277, 310 Main St., New Rochelle, N.Y.)

■ SPHERE GAP ASSEMBLY, model 8526, for high-voltage measurement, includes a sphere gap and current-limiting resistors mounted on an insulating base. Spacing of the 6.25-cm spheres is controlled by a micrometer adjustment calibrated in mils. Measurement accuracy is said to be ±3 percent for d-c potentials and a-c peak potentials at commercial power frequencies up to 120 kv. (Associated Research, Inc., Dept. Sci288, 3777 Belmont Ave., Chicago, Ill.)

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■ AMPLIFIER for measurement of nuclear radiation recovers rapidly after a large pulse of several hundred times overload. Rise time is less than 0.2 μsec. Gain stability is ±0.027 percent with ±1 percent change in line voltage. Linearity over the 0-to-100-volt range is better than 0.1 percent into loads greater than 1000 ohms. Gain is 10,000 with delay-line clipping and 20,000 with *RC* clipping. (Hanmer Electronics Co., Dept. Sci279, Princeton, N.J.)

■ MULTICHANNEL OSCILLOSCOPE uses a 17-inch, long-persistence cathode-ray tube to display 24 channels of information. Each of 24 plug-in channel amplifiers has its individual control for vertical position, beam intensity, gain, and balance. Sweep is calibrated from 100 μsec to 1 sec/cm. Frequency response is d-c to 3500 cy/sec. Sensitivity is 10 mv/in. (Railway Communications, Inc., Dept. Sci280, 9351 E. 59 St., Raytown, Mo.)

■ SPECTROPHOTOMETER ACCESSORIES include precision-bore 10-mm diameter cuvettes and "micro-space" adapters. Used alone, the cuvettes require a minimum sample volume of 1.8 ml. The adapter reduces the volume requirement to 0.7 ml while retaining the 1.0-cm path. Cells are matched to 0.5-percent transmission. (Coleman Instruments, Inc., Dept. Sci284, 42 Madison St., Maywood, Ill.)

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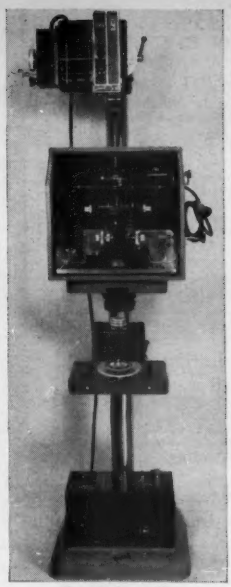

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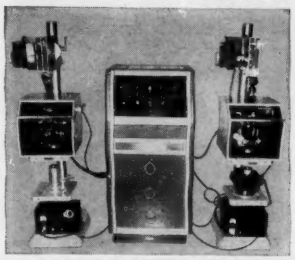

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Two copies of the application should reach the Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1, not later than **31 January 1960.** An additional copy should be sent direct by airmail to the Registrar, University of Cape Town, Private Bag, Rondebosch, Cape Town, South Africa, by the same date.

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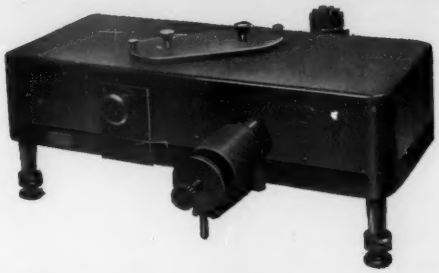

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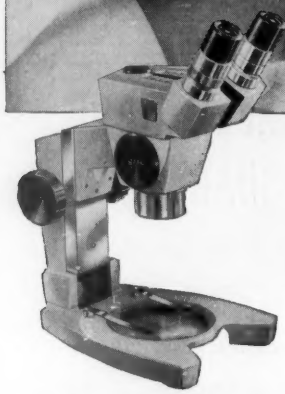

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